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[*October* 8, 1934]

Some Milestones of Achievement in Army Hygiene and a few Suggestions for further Progress

PRESIDENT'S ADDRESS

By Major-General P. H. HENDERSON, C.B., D.S.O., M.B., Ch.B., K.H.P.

I REGARD the charter of the Army Hygiene specialist to be the conservation of the man-power of the Army through the maintenance and promotion of health and the prevention of disease. The charter, therefore, embraces everything which affects the health and well-being of the soldier and also of his family, for, if the married soldier has not got a well-housed, well-fed, happy and healthy family, his morale and health, and with them his work, will suffer. I propose to confine myself chiefly to those milestones in hygiene which affect the soldier's environment and life in barracks and, to a less extent, in the field. The marked success of certain other measures in preventive medicine, such as inoculation, have received more public notice in the past, although the subjects with which I will deal are of equal importance, but, because they are constantly before us, any progress in regard to them is apt to be taken for granted.

I should like to say that such progress as has been made in regard to Army Hygiene is very largely due to the initiative of hygiene officers and to the close co-operation maintained between the Hygiene Directorate (which embraces the hygiene specialists in Commands, Districts and Educational establishments) and other branches of the Army, and more particularly with those branches concerned with engineer services, and the feeding and housing of the troops.

MILESTONES

(1) *Education.*—Since I joined the Army some thirty-five years ago, education in regard to matters of hygiene and sanitation has improved in a very marked degree. Most of this improvement has taken place since the World War, largely as the result of the formation of the Hygiene Directorate which was created under Army Council Instruction No. 545 of September 16, 1919, and the valuable support this Directorate receives from the Army Hygiene Advisory Committee. Since the formation of this Directorate, not only has the teaching of hygiene and sanitation at the R.A.M. College and Army School of Hygiene been re-organized on up-to-date and thoroughly practical lines, but Commands and Districts are now supervised by fully qualified specialist officers, and £200 is at the disposal of the Director of Hygiene annually for purposes of research—some of the valuable fruits of this research will be referrrd to later.

Under the duties for which the R.A.M.C. is maintained, first place is given in the R.A.M.C. "Training Manual" to "maintenance of health and prevention of disease," and "the latter" is also given priority in the general duties of the Medical Services in the Army Regulations. That is as it ought to be, and it is pleasing to

note that in civilian life there is a strong movement afoot to follow our good example in this direction, and thus bring the practice of medicine throughout the country into its true perspective.

The education of officers and other ranks of the Medical Services of the Army in hygiene and sanitation is carried out at the R.A.M. College and Army School of Hygiene, and that of officers and other ranks of other arms at the Army School of Hygiene. The education of the combatant branches is also carried out in all Stations by means of lectures delivered during the individual training season, and—perhaps most important of all—by practical demonstrations during inspections of barracks and camps by properly trained hygiene specialists. What is the result of all this? It is that the majority of combatant officers, who, when I first joined the Service, took little interest in matters affecting the health and well-being of their men, now take a genuine and live interest in all these problems with gratifying results.

In testimony of this it was claimed a short time ago by that eminent biographer, Sir George Arthur, that the recent improvements in the soldier's environment, particularly in regard to messing and accommodation, were due solely to the personal efforts and initiative of the present-day Commanding Officers of Units. Now, while a scrutiny of the official files would probably reveal the fact that these improvements originated in recommendations made by hygiene officers, it is a great satisfaction to know that the new interest taken in such important matters by combatant officers should induce this well-known writer to make such a claim, and it is certain that, because of the greatly increased knowledge of hygiene possessed by staff and other officers, it is essential, if medical officers are to continue to be in a position to give advice on this subject to these officers, that they should keep themselves thoroughly up to date.

(2) *The housing of the troops.*—Up to the middle of the nineteenth century the housing of the soldier was deplorably neglected, and inquiries instituted just before and after the Crimean War showed that the high mortality from pulmonary diseases prevailing at that time was due to the overcrowding of men in insanitary barrack-rooms. To give but two examples: in England in 1820, the beds were arranged in double tiers, and in Jamaica the men slept in hammocks touching each other.

It was not until 1851 that a definite cubic space of 450 cubic feet per man in barrack rooms at home was fixed. In 1855 Lord Monck's committee investigated the conditions then prevailing, and it is interesting to note that the motive of that committee was identical with ours to-day, namely, a desire not only to raise the standard of health and well-being of the soldier, but also to improve the tone in the barrack room, and thus produce better fighting material.

The chief recommendations of that and later committees were to increase the cubic and superficial areas allotted to each soldier, and the construction of separate married quarters, although they also made other suggestions for improving the environment of the soldier which were not acted upon until much later.

In 1857 a Royal Commission was assembled under Sidney Herbert, and this Commission found that the death-rate among soldiers in peace time was twice as high as among the civilian populations of the same ages, the Army death-rate being then recorded as 17·5 per 1,000. In 1933 the death-rate for the Home Army was 1·87 per 1,000 and that for the whole Army at home and abroad 2·15 per 1,000, while the civil death-rate for England and Wales during the same year was 12·3 per 1,000. As the result of the Royal Commission's recommendations in 1857, the space per man was raised to 600 cubic feet at home and 800 cubic feet abroad. A survey of the barracks in the United Kingdom at the time showed that, based on this allowance, they fell short of the necessary accommodation by about one-third. The important factors of floor space and the space between beds were not definitely laid down until a much later date. Although not regarded so at the time, a most important

recommendation of the Royal Commission was the utilization of medical assistance in the selection of sites for barracks and in other matters affecting the soldier's environment.

The Barrack and Hospital Improvement Commission was next formed to consider in detail the recommendations of the Royal Commission and in 1862 was constituted a Standing Committee. In 1865 their title was changed to that of "The Army Sanitary Committee," which remains a very active body to-day under the title of "The Army Hygiene Advisory Committee," presided over by the Director of Hygiene. The duties of this Committee in regard to barracks are as originally laid down: viz. (1) To report on new sites and on block plans of new barracks—making whatever inspections may be necessary; (2) to report on all type plans, especially with reference to sanitary details; (3) to report on all new principles of construction or fitments from the hygienic aspect. The recommendations of this Committee in regard to housing are later dealt with by the Barrack Synopsis Committee on which the Directorate of Hygiene has a representative.

When we reflect on the conditions under which the soldier was housed in the past, of which I have given you but a brief summary, and when we inspect some of the barracks and huts still in existence which were erected at the time of the Crimean War or a little later, we can congratulate ourselves that, as a result of close coöperation between the Hygiene Directorate and the Engineer Services, we have definitely reached a milestone of achievement in the latest barrack design.

In contrast to many of the older barracks—too many of which are still in existence—in which the rooms, each accommodating from twenty-four to thirty men, are square-shaped, the windows, equipment shelves, fireplaces and beds are badly placed, and the floors made of soft wood, the latest design of barracks in the United Kingdom shows the following important improvements. It consists of two-story blocks containing four barrack rooms (two on each floor). On each floor one of the rooms holds 13 men and the other 17 men. There is also a N.C.O.'s room and, leading off from between the two barrack rooms, a sanitary annexe containing ablution basins, W.C.s and urinals, all under cover.

The rooms are oblong in shape, and the beds and windows are arranged in sequence as follows: A bed next the end walls, then a window alternating with two beds throughout the length of the room. This ensures each bed having a window at one side of it. Windows on either wall reach to near the ceiling and face each other, and are of adequate dimensions to permit of good cross ventilation. Each bed has 60 sq. ft. floor space and 600 cubic feet air space, and there is 6 ft. linear wall space between the centres of beds. Metal equipment boxes are placed over the correct position of each bed. Floors of hard wood suitable for polishing are provided. Heating is still by coal fires placed back to back in the centres of the rooms, but though this method of heating is cheerful and the men like it, it has the grave hygienic objection that in cold damp weather men tend to congregate round the fires, and so disseminate such dangerous infections as cerebrospinal meningitis and influenza.

The ideal method of heating, in my opinion, is properly distributed panel heating, as this obviates the risk involved by men congregating, as they do around fires, and also their tendency where central heating is provided, to dry dirty wet clothes on the hot pipes or radiators and thus create a very unpleasant hot, humid atmosphere.

The reduction in the numbers of occupants from 24 or more to 17 and 13 greatly facilitates segregation in the presence of epidemic disease, thus limiting its extent.

(3) *Married Quarters.*—In 1855, when Monck's committee were investigating the condition of barracks, married soldiers, up to 6% of the strength of the unit, lived in the barrack rooms with the single men, and were separated from them only by blankets hung across the room or by curtains round the beds. In some cases even this apology for decency did not exist.

The first improvement of these conditions was the allotment to married soldiers of barrack rooms apart from the single men, but with several families in the same room. Such conditions continued up to the 'seventies, when separate quarters were built in the form of single rooms, arranged four on a floor, in a building of two or three stories in height. Some of these single "Y" quarters, as they were later called, were in existence up to a few years ago. Then followed the three-roomed type, consisting of a living room, bedroom, and scullery, to be superseded later by the barrack attic type and the depot type with more rooms and in which the larger quarters were allotted according to *rank*.

Up to now, W.C.s were detached from the quarters. Then came the verandah type in which the W.C.s were provided at the ends of the blocks, and about this time also quarters were allotted according to the size of the soldier's family and not by rank.

About thirty years ago statistics were taken, and as a result married soldiers' quarters were reduced to three types without distinction as to finish or as to the rank of the occupant. These were designated "A," "B" and "C" type quarters and had one, two, and three bedrooms respectively and each had a living room and scullery.

The next improvement was the addition of a W.C. to each quarter and the provision of a Sitz bath, and since the World War and as the result of recommendations made by the Hygiene Directorate in coöperation with the Royal Engineers, each new quarter is now provided with (in addition to the living room and bedrooms) a W.C., scullery with hot water laid on to the sink, a larder, bath with hot and cold water, a coal store, a wardrobe fixture with extending hanger for clothes in each bedroom, and in the best bedroom a cupboard for rifle and accoutrements. Drying posts may be provided on the scale of two per quarter, and where suitable arrangements can be made with a gas company, permission is given for the installation by the company of a cooker and meter in the scullery.

In all three types of quarters the floor space of the living-room is 160 sq. ft., and the bedroom in the "A" type has a similar measurement, while in the "B" and "C" types the best bedroom measures 150 sq. ft., and the second bedroom 100 sq. ft., the third bedroom in the "C" type being 90 sq. ft.

My only adverse criticism of the new quarters now being built is that the space from floor to ceiling is only 8 ft., which conforms to the minimum and normal measurement followed by the Ministry of Health in the construction of working-class dwellings. The Department of Health for Scotland, however, more nearly supports my desire for a 9 ft. minimum. In their two-story cottages they provide 8 ft. 6 in. on the ground floor and 8 ft. on the upper floor, while in flatted or tenement houses of two or three stories they provide 8 ft. 6 in. on all floors, and in certain instances 9 ft. on the ground floor. Bearing in mind the habits of the occupants, in my opinion, no matter how the windows are placed, anything under 9 ft. results in insufficient ventilation.

All quarters are now allotted not only according to the size of the family, but also according to the age and sex of the children. Having secured satisfaction in regard to the principle on which quarters are allotted and the hygienic aspect generally, the next thing was to see that sufficient quarters were provided of the appropriate sizes to meet the requirements of the soldiers' families as they actually exist in the average unit. For this purpose representations were made to the Quarter-Master-General's branch in regard to overcrowding, and a census of the families and quarters in different stations was taken in 1923 and again in 1930. As a result the proportion of "A," "B" and "C" quarters has been very materially altered so as to provide more adequate accommodation for the larger families found to exist by census.

The following table gives approximately the proportion of "A," "B" and "C"

quarters existing in 1911 and provided in 1923 and 1931 in accordance with the census returns :—

	"A"	"B"	"C"
In 1911 Service units ...	60%	30%	10%
In 1923 ,, ,, ...	50%	35%	15%
In ,, Depots	40%	40%	20%
In 1931 Service units ...	45%	35%	20%

(4) *Feeding of the soldier.*—Of all the milestones to which I shall refer, none marks greater achievement than in the feeding of the soldier, and under the feeding I include not only the quality and quantity of his food, but also its cooking and service, the buildings provided for its preparation, and those in which it is eaten.

Diet.—An attempt to trace in detail the stages by which our present milestone has been reached would occupy several hours. I will skip the stages which intervene between 1660 and 1921. In the former year no food was provided for the soldier, and a Private of Foot was paid but 8d. a day, and from this sum he had to pay the innkeeper on whom he was billeted 4d. per diem for lodging, food and beer. In 1921 the Treasury accepted the principle that the public must be responsible for feeding the soldier, as is the practice in war. Here I would say, in passing, that no troops were ever so well fed in war as were ours during the World War, and no factor was of greater importance in maintaining a high standard of health and morale.

In 1921 the ration provided by the State consisted of three parts :—

(1) That issued in kind by the R.A.S.C. : Meat (frozen or fresh), 12 oz. ; bread, 16 oz. ; tea, $\frac{8}{9}$ oz. ; sugar, $1\frac{1}{2}$ oz. ; salt, $\frac{1}{4}$ oz.

Periodically, for the purposes of mobilization, turnover 9 oz. preserved meat and 12 oz. of biscuit are issued in lieu of the meat and bread ration, and on these days a small cash allowance is credited to the messing account for every 1 lb. of each conserved. Underdrawals of the R.A.S.C. portion of the ration are not allowed except in the case of bread. For every 1 lb. of bread underdrawn, 12 oz. of flour may be drawn in lieu for making pastry, cakes, etc.

(2) That supplied by N.A.A.F.I. at contract rates and on which no rebate is paid : Bacon, 2 oz. ; cheese, 1 oz. ; jam and syrup, $1\frac{1}{2}$ oz. : margarine, 1 oz.

(3) A cash allowance of $3\frac{1}{2}$d. In 1928 this cash allowance was reduced to 3d., but in 1931 a change of policy was introduced whereby the cash allowance now varied between a maximum of $3\frac{1}{2}$d. and a minimum of 3d. The allowance is reviewed monthly and is based on the cost-of-living index of the Ministry of Labour.

The cash allowance is made with the object of enabling units to buy such extra articles of food from the N.A.A.F.I. as will ensure the provision of a well-balanced and varied diet, containing all the essential constituents in proper amounts and proportions. The hygiene specialist must scrutinize, from time to time, all diet sheets exhibited in the cookhouse to satisfy himself that this object is achieved. The principal articles bought from the cash allowance are vegetables, milk, liver, fish, eggs and such like. Should the cash allowance prove inadequate for the purchase of these articles in sufficient amounts, units may commute for cash the cheese, jam, margarine, and up to 6 oz. weekly of the bacon, but commutation to any great extent under the existing system is not a wise procedure, nor is it made use of to any great extent, as the bacon, cheese, jam and margarine are bought at contract rates and provide excellent value for the money expended, whereas if these articles are commuted, the commutation is done at wholesale rates and fresh purchases have to be made at retail rates and result in a bad bargain.

Even the soldiers' Christmas dinner (New Year's dinner in Scottish Units) receives consideration nowadays, and a halfpenny for each man actually in mess may be taken from the units' credit balances and spent on this special Christmas meal.

As the result of recent propaganda much more whole fresh milk is now being used in substitution for tinned milk, and the milk supplied under contract to hospitals is pasteurized.

The peace-time diet which I have just described contains in sufficient amounts and in proper proportions the proximate principles—proteins, fats, carbohydrates, vitamins and mineral salts—and has an average caloric value of approximately 3,600 calories. That this diet is satisfactory is proved by the physique of the average soldier and the health of the Army, and by the improvement in the physical and mental state of the recruit at the end of his depot training. The average recruit gains 8 lb. in weight during that time, and many gain fully twice as much, while there is a marked advance in the standard of education of the vast majority.

The feeding of the territorial soldier and of the cadets of the Officers Training Corps while in camp receives careful supervision on similar lines to that exercised over the ration of the regular soldier, and has a value of over 4,000 calories.

When I was Assistant Director of Hygiene, Eastern Command, I observed that, notwithstanding the excellent organization and administration of the Duke of York's Royal Military School at Dover, the boys were stunted in growth. I therefore made a careful investigation of the feeding of the boys, and discovered that the dietary was lacking in several essentials for growth. As the result of my representations the feeding of these boys has been greatly improved by the inclusion of one pint of fresh whole pasteurized milk daily per boy. Eggs, fish, butter and a more liberal supply of fresh vegetables are also provided. The feeding of the boys in Queen Victoria's School, Dunblane, has been improved on similar lines, although, owing to extra cost, the milk is not pasteurized.

This year approval has also been obtained for each boy at the Army Technical School, Beachley, to receive a daily allowance of 5 oz. of whole fresh milk and 15 oz. of hand-skimmed milk.

Quality of food.—The quality of the different items of food forming the ration is controlled by means of carefully drawn up specifications, inspection by experts and analyses at the Central Reserve Depot and R.A.M. College, and also by the provision of suitable transport and storage facilities, including, in some hot stations, a supply of ice boxes and ice. The regulation of the supply of refrigerators and ice in all stations where the temperature normally exceeds 80° F., is at present under consideration.

Housekeeping.—An important subject, formerly neglected, is now carefully supervised by the Inspector of Army Catering on behalf of the Director of Supplies, and is controlled by Unit Messing Committees which contain representatives of all ranks. It is also greatly facilitated by the training of officers and cooks at the Army School of Cookery.

Valuable guidance is obtained from various manuals, most of which are compiled by the Inspector of Army Catering and are issued to units. These include the Manual of Military Cooking and Dietary, the Management of Soldiers' Messing, Rules for the Conduct of Regimental Institutes, War Office Quarterly Specimen Diet Sheets, Army Book B. 48 with lift sheets, etc.

Cookhouse and dining-hall buildings.—All the more senior Army officers are familiar with the Army cookhouse as it existed about the time of the South African War. This type still existed in some barracks until fairly recent years, and consisted of a small building situated between two barrack blocks. This building contained the cookhouse on one side of a central wall and the latrines on the other. There was no preparation room or proper store cupboards. One shallow sink only was provided in which vegetables, cooking pots and other dirty articles were washed indiscriminately. There was no servery, hotplate, dining hall, or wash-up room. The food, often badly cooked by untrained cooks and most usually consisting of a greasy stew, had to be carried a considerable distance to the barrack rooms, where it frequently

arrived cold, to be eaten there. After the meal the crockery and cutlery were cleaned (*sic*) in a pail of luke-warm water, dried on the soldier's body towel and stored, until the next meal, along with his bedding or kit. Any bread or other food not used was stored in cupboards in the barrack rooms or thrown away into the swill tubs. What a change we find to-day! The whole of the buildings concerned with the feeding of the soldier have been redesigned, and now consist of a separate block containing: (*a*) Preparation room with suitable cupboards, food preparation benches, tables, deep sinks, etc.; (*b*) kitchen with up-to-date cooking apparatus, deep sinks in which to wash cooking utensils and, hung up in a prominent place, the menu of the week, rules for cookhouses and a list of the cooks employed with particulars as to their health; (*c*) a cupboard outside the cookhouse for the cook's special clothing and ordinary uniforms: (*d*) a servery with hotplate; (*e*) a dining hall suitably heated, lighted, furnished and equipped; (*f*) a wash-up room with mechanical washers and cupboards in which to store the feeding utensils.

All these rooms are under one roof and communicate with each other, and in well-administrated units provide ample facilities for the efficient preparation, cooking and serving of excellent meals in pleasant surroundings.

All new barracks are now provided with this building, and in most of the older barracks an effort has been made by reconstruction to bring existing cookhouse buildings as nearly as possible into line with this plan.

Preparation and cooking of the food.—Although there is still a paucity of trained cooks in some units, an effort is now made to supply to each unit thoroughly trained cooks who have received their training at the Army School of Cookery. I have had the privilege of inspecting and sampling all varieties of food cooked by these men at the conclusion of their Course of Instruction, and I endorse the opinion I have heard expressed by eminent civilian experts, who very kindly judge the different items cooked under Army conditions at the end of the course, to the effect that the preparation and cooking has improved enormously in recent years and that the work of these cooks compares very favourably with that of expert civilian chefs.

First-class cooks now receive 6d. a day extra pay and second-class cooks 3d. a day from Army Funds, instead of from Regimental Funds as was the practice prior to 1934. As the result of having trained cooks and messing committees the everlasting stew is now a thing of the past, and the menus are well varied from day to day and week by week.

Serving of the food.—No matter how well food may be cooked, unless hot food is served hot and the meal is partaken in clean and attractive surroundings, little pleasure is derived from eating and much of the value of the food is lost. The hotplates now installed in the servery provide the means for keeping food and dishes warm and in most, if not all, units the other essential points receive careful attention.

(5) *Cleanliness of the soldier.*—Monck's Committee in 1855 recommended the introduction of ablution rooms and foot baths, but this recommendation was not acted on until much later.

When first provided, the ablution rooms were equipped with cold-water taps, slate benches and detached metal basins. These basins have now been replaced by fixed glazed earthenware or enamelled iron basins which are more hygienic, but hot water is not yet laid on, except in certain workshops. I hope before long this obviously hygienic necessity will be supplied to all ablution basins.

So far as I am aware baths first appeared in Barrack Synopsis in 1865 when slate slipper-baths were authorized on a basis of 1% of the strength of the unit. The 1898 Synopsis showed hot and cold water laid on and the baths painted inside and out. In 1910 The Army Medical Advisory Board, as the result of a meeting of the Construction Sub-Committee, recommended the provision of 4% combined foot and shower baths in addition to the 1% slipper-baths. At the same time

Major Horrocks, the hygiene representative on that committee, recommended that in each bath house one slipper and one combined foot and shower bath should be provided for N.C.O.s. These recommendations are in force to-day, but the fuel allowance rarely admits of more than one hot bath a week. Last year I proposed to the Army Hygiene Advisory Committee that, for private soldiers, all slipper-baths should be replaced by combined foot and shower baths on the grounds that shower-baths are much more hygienic than slipper-baths and are more economical in fuel and water, and because of the latter advantages, baths could be given more frequently than at present without increasing the cost, a very desirable procedure for combating scabies and other skin infections which are so closely associated with dirt, and which cause much absence from duty and consequent expense to the State. My recommendations are under consideration by the proper authorities.

Swimming baths are becoming more popular and are provided in a number of Stations at home and abroad. We are at present engaged in making analyses of all Army swimming-bath waters at home, with the object of ascertaining the best means of purification. At present some are without any proper means of purification, while in others purification is carried out by means of filtration, chlorination or chloramination and aeration. Owing to the prevalence of middle-ear disease, no man with discharging ears is allowed to use Army swimming baths.

From these remarks it is clear that while we have made considerable progress on the road to cleanliness we have still some distance to go before we reach the desired milestone.

(6) *Mother and child welfare.*—No achievement is likely to prove of greater value to the married soldier and his family than the official recognition of Army Mother and Child Welfare Schemes and Centres. I had the honour of representing the Medical Services on the War Office Committee which threshed out this subject, and of pressing for the official recognition of Army Welfare Schemes, and was very gratified when approval of the schemes and the establishment of Welfare Centres up to a total of seventy was given in 1924.

The Treasury grant for the upkeep and running of the centres, viz. £6 per centre, with a little special furniture for centres established in official medical inspection rooms for women and children, is very small compared with that given to civilian centres, but by the aid of extra money raised by units very creditable centres have been established in most stations where they are required.

A special form, Army Form C. 320, is prepared for each child up to 5 years of age, and on this a confidential record is kept of all disabilities from birth up to school life, where the School Form, Army Form C. 319, comes into use.

Where the centres are properly run and advantage is taken of the services of the specialists in Districts and Commands, no soldier's child should grow up with a disability which is capable of prevention, alleviation or cure.

When the children reach school age and go to the Army schools, four sizes of school desks, constructed on hygienic principles, are provided for the older children, and two special sizes of chairs and tables for infants, with the object of preventing postural deformities, and every attention is given to the lighting, ventilation and other amenities in the schools to ensure good eyesight and general good health.

In regard to the mothers, the ideal aimed at is close coöperation between the centres and military families' hospitals. The specialist in women's diseases, where available, gives ante-natal and post-natal advice at the hospitals. All women are encouraged to come into hospital for their confinements, and most do so, with the gratifying result that, whereas the maternal mortality in the civil community is in the neighbourhood of 4·5 per 1,000 of confinements, in the Army during the latest three years for which figures are available—1930 to 1932—it has only been 0·37, 0·71 and 0·37 per 1,000 respectively.

Instructions given by the specialist in regard to the ante-natal and post-natal periods are carried out at the centres and also in the soldiers' homes, under the supervision and guidance of nurses belonging to the Soldiers', Sailors' and Airmen's Nursing Association. The centres are run by committees composed of the wives of all ranks under the special supervision of medical officers, assisted by the nurses. At the centres, in addition to medical examinations, weighings, &c., valuable advice and instruction is given in regard to feeding, clothing, upbringing of children, and other useful matters, and special infant foods are sold at reduced prices.

I need say no more to convince you of the value of this organization from the health aspect, but it has also another very important use in that it brings together the wives of officers and other ranks and creates a friendly interest between them, which goes a long way in fostering a contented and happy unit and military community.

(7) *Supervision of physical training from the physiological aspect.*—An important milestone in Army training was reached in 1922 when the first Hygiene Specialist was appointed to the Staff of the Army School of Physical Training, to supervise that training from the physiological aspect and to carry out research in regard to this aspect of physical training. The officer appointed to this post receives special training in physiology in the Hygiene Department of the R.A.M. College, a course of experimental physiology under Professor Cathcart, in Glasgow University, and passes through special courses of physical training at the Army School before taking up the appointment. Since the post was created the duties of the incumbent have been increased, and now include the teaching of anatomy and elementary physiology to officers and prospective physical training instructors, and also of first aid and remedial exercises to the latter. He is also in medical and sanitary charge of the school. Much valuable research has been done by these hygiene specialists in regard to (1) the effects of smoking on endurance and on short bursts of energy as in sprinting; (2) the energy expenditure entailed in the physical training exercises at present in use; (3) exercise tolerance before and after training; (4) strength of grip before and after training; (5) effects of abdominal breathing on vital capacity, &c. Stated very briefly, this research has proved that smoking is detrimental to endurance, but does not seem to affect sudden bursts of energy as called for in sprints. The energy tests showed that our present exercises and tables, although selected and arranged as the result of experience but more or less empirically, are, on the whole, arranged in the proper order of progression as regards energy expenditure, and are admirably suited for the production of high mental and physical standards. Exercise tolerance and grip both improve in a satisfactory manner as the result of the training received and deep abdominal breathing gives a greater increase in vital capacity than the customary chest breathing.

A recent innovation, which will have a valuable bearing in the supervision of physical training in units and depots from the medical aspect, is the introduction of special courses at the Army School of Physical Training for all R.A.M.C. officers on joining the Service and for Hygiene Specialists.

In the Field

(8) *Intelligence.*—Under this title I wish to deal particularly with Military Medical Intelligence, and I am sorry to say that we have not yet reached that milestone on the road to success which is desirable if we are to avoid the errors and losses of previous campaigns. To achieve success in dealing with health problems it is essential to know what problems you have to face, and to have schemes ready for application at the commencement of a campaign. With this object in view one of the first things I asked my Director to allow me to do when I joined the Hygiene

Directorate in the War Office in 1923 was to build up a Hygiene Intelligence Bureau from which could be obtained reliable and up-to-date information in connexion with all matters likely to affect the health of troops in any country. He welcomed the suggestion, so I interviewed the Director of Military Intelligence and the officer responsible for the military handbooks and through these officers formed a close liaison with the Intelligence Branch. I drew up for the branch a pro-forma setting forth the various headings under which we desired to obtain information and on which the special chapter dealing with such matters in these handbooks should be based. I also suggested that we could get much valuable information through the Army Intelligence representative on the League of Nations and the Ministry of Health representative on the Health Bureau of the League. These suggestions were all agreed to and are in operation to-day. We obtain from the annual and other reports of hygiene specialists, reliable information of the nature required in regard to those parts of the Empire in which these specialists are stationed, and while officers of the intelligence branch serving in different countries do their utmost to help us, the technical nature of the medical information we require makes it extremely difficult, if not impossible, for the latter to provide us with this information, and I often wonder if we have gone far enough or whether we should not have a hygiene specialist in the Intelligence Branch.

(9) *Feeding.*—To overcome the monotony of the daily stew or of cold bully beef has always been an important and difficult problem in the field, but it has now been solved by the introduction of travelling cookers using petrol or other form of oil, and by the description in the Army Manual of Cooking and Dietary of many different ways of cooking field rations.

(10) *Drinking-water.*—Those who have had experience in the field since the introduction by Colonel Sir William Horrocks in 1914, of the chlorine method of purification of water, which must have saved many lives and prevented much sickness, while appreciating the value of that method, must also have been aware of its disadvantages in regard to the instability of the bleaching powder used, the deviation of the chlorine in the presence of organic matter, and the unpleasant taste imparted to the water and to tea by the chlorine when the process was not very strictly controlled. In order to overcome the instability of the bleaching powder, this powder was replaced in 1921 by the more stable "Water Sterilizing Powder" containing a mixture of quicklime and bleaching powder. This change was introduced chiefly as a result of investigations carried out by Major Elliott at the R.A.M. College.

In 1922, Major Harold and Captain Ward commenced their important work at the Army School of Hygiene on chloramines formed by the interaction of chlorine and ammonia. Captain Ward having left the School, Major Harold continued the investigation and in 1924 and 1925 he published the results of this most valuable piece of research, recommending the substitution of chloramine for chlorine in the purification of water in the field.

He got better results with preformed chloramine than with bulk-formed chloramine, but as the production of the preformed variety entailed the use of sparklets containing chlorine gas and a syphon, Major McKibbin continued the investigation at the School in the hope of devising a simpler process, and succeeded in getting good results in purification with *bulk-formed* chloramine. This process had the practical advantage over Harold's method of doing away with the necessity for extra apparatus. In 1931, Major Elliott, acting on the above principles, devised an admirable purification plant erected on a platform and carried on a motor chassis, with a total weight of about one ton. This plant is known as the "Elliott Mobile Water Purifier" and is capable of drawing water from a depth of eight feet, chloraminating and filtering it, and delivering it to tanks to a height of twenty feet, at the rate of 1,200 to 1,500 gallons an hour.

In Elliott's Purifier chlorine is produced by the electrolysis of salt water. He and McKibbin found that best results are obtained by using ammonia and chlorine in the ratio of 1 to 4, and that for practical purposes in the field the chloramine should be applied to the water in the proportion of two parts per million. Using this strength a safe and tasteless water is obtained after one hour's contact. In this concentration also chloramine has the advantage over chlorine of killing the cercariæ of bilharzia in clarified water in half an hour and in crude water in one hour. Elliott's and McKibbin's methods of purification have been tried out during manœuvres under active service conditions, with satisfactory results. McKibbin's method has also been tested in Egypt with equally good results.

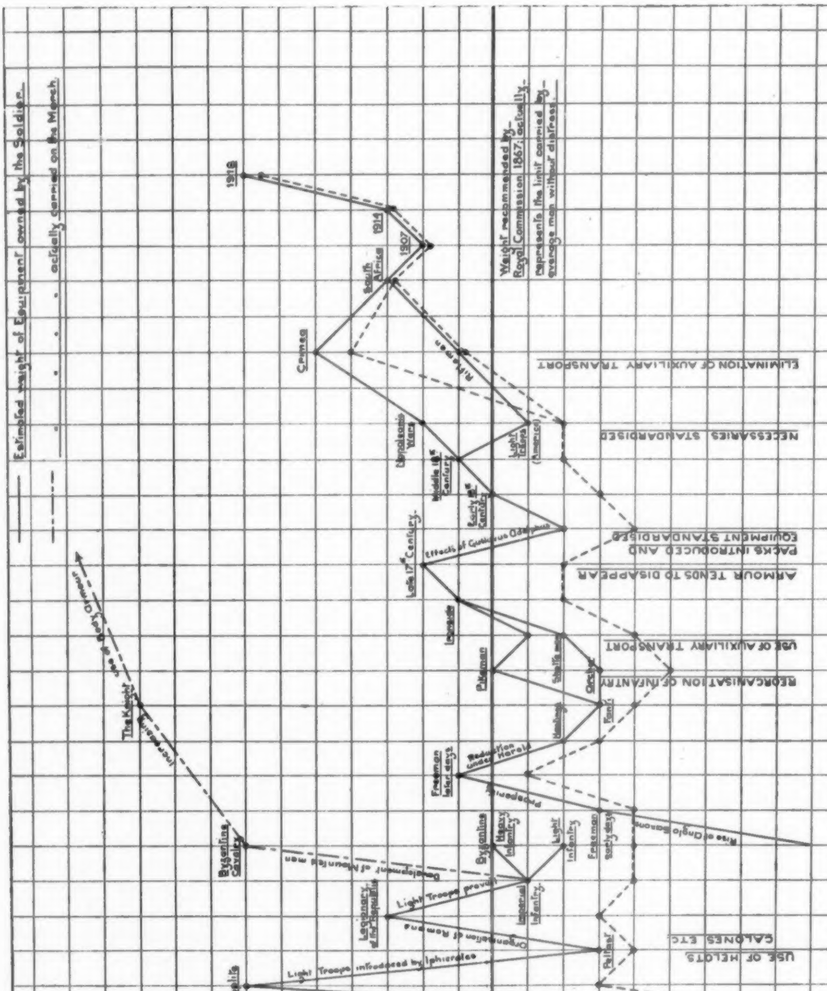
(11) *Clothing and equipment.*—Those of us who served for a prolonged period with front-line troops in the World War observed how greatly handicapped our marching units and particularly our infantry were by being overloaded, and by the unsuitable nature of their uniforms, which constricted the neck, interfered with the circulation of the blood and with the free circulation of air around the body. The combination of these factors induced men to discard essential parts of their equipment or produced early fatigue, with the result that they could neither fight to the best advantage nor adequately defend themselves. As a result many were killed or wounded who, if they had been properly clothed and equipped, would doubtless have fared very differently. Many men also succumbed to "trench foot," a disability induced, to a great extent, by tight wet puttees.

The Hygiene Directorate took up this matter soon after the War.

In 1921, Major Norman Lothian, a most able officer of the Directorate, who, later, when Secretary of the Health Bureau of the League of Nations, met an untimely death through a motor accident in Palestine, compiled a brilliant treatise on the load of the soldier, and traced the vagaries of that load from the early days of the Greeks and Roman Legions through history up to and including the period of the World War. The striking lesson which Major Lothian's pamphlet taught was that we had failed in most of our wars to benefit from previous experience.

It usually happened that at the end of a war it became obvious that the soldier was carrying a too-heavy burden, and when the war was finished an attempt was made to reduce the fighting load, but the lesson was soon forgotten and when the next war was declared the infantry soldier was again converted into a beast of burden.

Time will not permit me to traverse the ground covered by Major Lothian, so I will merely refer you to his chart on p. 12 and confine my remarks to the South African and World Wars. During the South African War the load was about 60 lb. and the soldier usually discarded a good deal of it, so an attempt was made, later in the campaign, to provide a special load of 25 lb. Following the war there was a revision of the equipment, and in 1907 the standard load was reduced from 60 to 54½ lb. In 1908 web equipment was introduced and the load went up to 59½ lb., and during the World War this load mounted up to 80 lb. in winter, and sometimes it actually reached 100 lb., an impossible burden for any animal of the weight of the average man. The table on p. 13 shows the body-weight and load of various animals and man. After the issue by the Hygiene Directorate of Major Lothian's treatise to the General Staff and other branches concerned, the Hygiene Directorate next took up the questions of (*a*) the energy expenditure of the soldier under certain loads and (*b*) the energy value of the ration. Research in regard to these important matters was undertaken by Professor Cathcart and officers of this Directorate, and their investigations were submitted to and considered by the Army Hygiene Advisory Committee in 1923. In regard to the load it was proved that the economic load of the soldier was approximately one-third of his body-weight.



MAJOR LOTHIAN'S CHART.

Animal	Body-weight		Load	Percentage of body-weight
Elephant	7,840 lb.	...	1,100 lb.	14%
Camel	1,120 lb.	...	300 lb.	22%
Cavalry (horse) ...	1,100 to 1,150 lb.	Rider 150 lb. / Equipment 115 lb.		24%
Mule	700 lb.	...	170 lb.	24%
Pony	700 lb.	...	170 lb.	24%
Bullock	700 lb.	...	170 lb.	24%
Donkey	450 lb.	...	100 lb.	22%
Man (coolie)	?	...	40-50 lb., but no fighting is required in addition to porterage	
Soldier	130 lb.		55 lb. 6¾ oz.	42·7%

The investigation of the energy-value of the ration gave a clear guide as to the calorie value of the ration required for the soldier's work under different conditions, and also as to the adequacy of the ration, tested as regards calorie value, for the work the soldier was performing.

Next followed experiments by the Hygiene Staff of the R.A.M. College to ascertain the best type of pack and the best position on the body on which to carry it. As a result the two-piece pack carried high on the shoulders, with steadying straps passing under the arms and quick release fastenings, was recommended. After that an investigation was carried out by the Hygiene Staff at the College in conjunction with Major Johnson, R.A.O.C., with the object of devising a more hygienic uniform for field service. The proposals of these officers were submitted to the Army Hygiene Advisory Committee for consideration, and the final recommendations of that Committee were forwarded to branches concerned and published in the Annual Health Report of the Army. As a result the C.I.G.S. ordered Commands to investigate the matter and submit recommendations.

When these were received the C.I.G.S. ordered the formation of the Braithwaite Committee, and gave the Committee instructions to consider the various reports and recommendations and decide on a suitable dress and equipment for the soldier for use on manœuvres and active service. Walking out and ceremonial dress was not to be considered. This Committee's choice, in most essentials, coincided with the recommendations of the Hygiene Directorate.

The new uniform and equipment has been on trial with two regiments since 1932, and reports so far are most favourable to the equipment, the hat, jacket with open collar, and the shirt with fixed collar, worn over the jacket collar to keep the latter clean. The wide peg-top trousers are preferred to the long shorts. No decision has yet been reached by the units in regard to the canvas gaiters owing to the very dry season and absence of mud. The short puttees, as expected, were found to be unsuitable.

(12) *Disposal of cookhouse sullage water and soapy water from baths.*—The efficient disposal in the field, during field training and in war, of these waters has always been a source of great trouble, owing to the soakage pits becoming blocked by a lining of grease or soap suds. After prolonged research by Major Hattersley at the Army School of Hygiene the problem would appear to be solved, for it is found that by adding copperas (ferrous sulphate) and lime to these waters, the grease and soap are deposited in a heavy precipitate in a very few minutes and the clear supernatant fluid can then be run off.

(13) *Disinfection in the field.*—A disinfector, using downward displacement current steam suitable for unit use, and capable of being transported on a pack animal, has now been tested and proved satisfactory. It was devised by Major T. O. Thompson at the Army School of Hygiene and fills a much-felt want.

(14) *The use of aluminium foil in the regulation temperature.*—Last year we had the privilege of listening to an address by Major Crowden of the London School of

Hygiene and Tropical Medicine, on his discoveries in regard to the properties of aluminium foil in reflecting and in preventing the radiation of heat, and the practical application of his discovery to the reduction of temperatures inside helmets, huts, tents, food containers, ambulance cars, etc. In the recent very hot summer he was able to carry out a field trial of his device during the annual training camp of the London University O.T.C. in the Isle of Wight, in regard to its application to food containers and tents. As a result he saved, in a fortnight, between £4 and £5 on the usual expenditure on ice alone by an initial outlay of £1 on foil. Food was easily preserved from deterioration, and the insides of tents where foil was applied, instead of being almost unbearable from the excessive heat, were compared to the shade of a tree covered with thick foliage The importance of his discoveries and their practical application in many directions in peace and war are very great.

Did time permit I could bring to your notice many more achievements in the field of hygiene but I hope my survey has been sufficiently comprehensive to show that we are achieving satisfactory results in many important directions.

Section of Therapeutics and Pharmacology

President—J. H. BURN, M.D.

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The Control of the Blood-pressure

PRESIDENT'S ADDRESS

By J. H. BURN, M.D.

ABSTRACT.—Present conceptions of the control of the general blood-pressure are based on the view that the sympathetic nerves are only constrictor in action and the circulating hormones, adrenaline and vasopressin, only pressor in effect. Surgical treatment of high blood-pressure and allied conditions has been directed accordingly to the removal of portions of the sympathetic system and to the removal of the suprarenal glands or tumours connected with them. Removal of portions of the sympathetic has been successful in relieving attacks of angina pectoris and removal of suprarenal tumours has relieved paroxysmal hypertension. Neither removal of portions of the sympathetic systems nor removal of the suprarenal glands has been successful in reducing continuous hypertension. It is suggested that our conceptions of the control of the blood-pressure need revision.

The normal variation in blood-pressure is not sufficiently realized. It may be as low as 85 mm. or as high as 190 mm. About one in every 40 men has a blood-pressure higher than 160 mm. It is suggested that the diagnosis of essential hypertension should never be made unless it is known that the blood-pressure has been rising. A single observation of a high pressure is not enough for the diagnosis.

Evidence is described that adrenaline and vasopressin may lower the blood-pressure as well as cause it to rise; similarly there is evidence that the sympathetic nerve supply to the skeletal muscles is dilator rather than constrictor in effect. Hence, all three mechanisms commonly thought of as pressor may also be depressor, and it seems more accurate to think of them as controlling the blood-pressure by raising or lowering it than simply as factors which raise it.

RÉSUMÉ.—Les idées actuelles sur le contrôle de la pression sanguine sont fondées sur la théorie que les nerfs sympathiques n'exercent qu'un effet constricteur et que les hormones circulatoires, l'adrénaline et la vasopressine, n'exercent qu'un effet presseur. Par conséquent, le traitement chirurgical de l'hypertension et des affections alliées consiste en enlevant des parties du système sympathique et des surrénales ou de tumeurs en rapport avec ces glandes. L'excision de parties du système sympathique a réussi à soulager les accès d'angine de poitrine, et l'excision des tumeurs des surrénales a soulagé l'hypertension paroxystique. Ni l'excision de parties du système sympathique ni celle des glandes surrénales n'a pu réduire l'hypertension continue. Nos idées sur le contrôle de la pression sanguine ont peut-être besoin de révision.

On ne se rend pas toujours suffisamment compte des variations normales de la pression sanguine. Elle peut descendre à 85 mm. ou monter à 190 mm. Environ un homme sur 40 a une pression sanguine de plus de 160 mm. L'auteur suggère qu'il ne faut jamais poser un diagnostic d'hypertension essentielle à moins de savoir que la pression est en train d'augmenter. Une seule observation de pression élevée ne suffit pas.

L'auteur décrit des raisons pour croire que l'adrénaline et la vasopressine peuvent diminuer la pression sanguine aussi bien que l'augmenter. Il y a aussi des évidences que les nerfs sympathiques des muscles squelettaux possèdent un effet dilatateur plutôt que constricteur. Il en suit que tous les trois mécanismes considérés d'habitude comme presseurs peuvent aussi être dépresseurs, et il semble être plus exact d'y penser comme contrôleurs de la pression sanguine en l'augmentant ou en la diminuant que simplement comme facteurs qui l'augmentent.

ZUSAMMENFASSUNG.—Der heutige Begriff der Beherrschung des allgemeinen Blutdruckes ist auf der Annahme begründet, dass die sympathische Nerven nur konstriktorisch und die Hormone des Blutstromes, Adrenalin und Vasopressin nur pressorisch wirken. Demnach hat sich die chirurgische Behandlung des Hochdruckes auf die Entfernung von Partien des

sympathischen Nervensystems oder von den Nebennieren oder ihrer Tumoren gerichtet. Die Exstirpation von Abschnitten des sympathischen Nervensystems haben Anfälle von Angina pectoris gelindert, und die Exstirpation von Nebennierentumoren hat den paroxystischen Hochdruck günstlich beeinflusst. Keine von diesen Methoden hat den dauerhaften Hochdruck herabgesetzt. Vielleicht müsste unser Begriff über die Blutdruckkontrolle nachgeprüft werden.

Die normale Veränderlichkeit des Blutdruckes ist nicht genügend erkannt. Er kann so niedrig wie 85 mm. oder so hoch wie 190 mm. sein. Der Blutdruck ist bei etwa 1 Mann auf 40 über 160 mm. Man solle nie die Diagnose von essentiellem Hochdruck stellen wenn man nicht weiss dass der Blutdruck sich erhöht hat. Eine einzige Beobachtung von einem hohen Blutdruck genügt nicht.

Verf. gibt Beweise dass Adrenalin und Vasopressin den Blutdruck virmindern ebenso wie erhöhen können, und es gibt auch Beweise dass die sympathischen Nerven der Skelettmuskulatur eher dilatatorisch als konstriktorisch wirken. Die drei Mechanismen die man gewöhnlich als Blutdruckerhöhend ansieht können also auch Blutdrucksenkend wirken, und es scheint die Annahme, dass sie den Blutdruck durch Senkung oder Erhöhen beherrschen als dass Sie ihn einfach erhöhen, richtiger zu sein.

IT is a commonplace of research that the rate of progress depends on the elaboration of quantitative methods. A method of measuring the blood-pressure was one of the earliest achievements of modern physiology, and yet, despite a great deal of work, our knowledge of the factors which control the blood-pressure can be summarized very briefly.

New observations have been made at three periods. In 1852 Claude Bernard and Brown-Séquard independently discovered the vaso-constrictor action of sympathetic fibres. In the few years preceding the close of last century Oliver and Schäfer [1] discovered the pressor action of extracts prepared from the suprarenal gland and from the posterior lobe of the pituitary body, and Bayliss [2] demonstrated the vaso-dilator effect of antidromic impulses transmitted along the sensory fibres. Until recent years there have been no further advances, and these observations still form the basis of theories of the cause of pathological changes.

The most important recent work has been directed to the examination of various depressor substances which have been discovered in the tissues. There is no need to speak to this audience of the work of Sir Henry Dale and Sir Thomas Lewis on the action of histamine as a vaso-dilator substance, nor of the work of Sir Henry Dale and his colleagues on acetyl-choline. Three other substances or groups of substances are perhaps less well known. Drury and Szent Györgyi [3] have demonstrated that a depressor substance in muscle is adenylic acid, and Drury has evidence that cytidylic acid is a second similarly acting substance. Drury is inclined to think that adenylic acid plays a part in causing the reaction to local injury, and that this reaction is not entirely due to histamine, for he finds that adenylic acid causes a migration of leucocytes when introduced into a rabbit's eye, whereas histamine does not. Kraut and Frey [4] have described a substance to which they give the name Kallikrein. It is present in extracts of pancreas which are insulin-free; its vaso-dilator effect is dependent on the reaction of the blood; in ordinary circumstances when the reaction is alkaline, kallikrein does not dilate the vessels; when the reaction becomes acid, however, kallikrein causes vaso-dilatation. Euler and Gaddum [5] have investigated extracts containing what they call "P-substance." These extracts cause a fall of blood-pressure in the rabbit after the administration of atropine; their action cannot be due to the presence of histamine or acetyl-choline, since histamine does not cause a fall of blood-pressure in the rabbit and since acetyl-choline does not act in the presence of atropine. The P-substance is present in intestine and brain.

Sir Henry Dale [6] has recently expressed the view that these depressor substances exist to provide mechanisms by which vaso-dilatation may occur locally, and not to exercise any control of the general blood-pressure.

Other work, which will be discussed in more detail, has shown that the pressor hormones, adrenaline and pituitary extract, are in certain circumstances depressor, and that the sympathetic nerves do not always exert a constrictor action.

Pathology of high blood-pressure.—As already suggested, this recent work has not, so far, affected clinical views of the cause of hypertension. At present it is usual to attribute high blood-pressure to the excessive action of vaso-constrictor agents: we think of blood-pressure as actively kept high, and allowed to fall passively. If, however, vaso-dilatation is also active, under the influence of depressor substances or nervous impulses, then a high blood-pressure may result from a diminution of the action of these substances or impulses, that is to say from a deficiency, rather than from an excess, of activity. This question is not only of theoretical interest, since the surgical treatment of high blood-pressure is increasing. When the physician bases his treatment on a faulty pathology, if he does no good, he usually does little harm; the surgeon, on the other hand, may do great harm, for what he does cannot easily be undone.

The surgical treatment of hypertension.—Not that the record of the surgeon in this field is one of mere failure, for there have been some striking successes in two directions. The first is in the surgical treatment of anginal attacks, and the second in the treatment of paroxysmal hypertension.

(1) Treatment of angina pectoris.—Jonnescu [7] treated angina pectoris by removing the cervical sympathetic chain on the left side from the lower end of the superior cervical ganglion down to and including the lower end of the inferior cervical or stellate ganglion. The accelerator fibres to the heart run from the stellate ganglion, and this ganglion also supplies the sympathetic fibres to the corresponding arm. Three cases were described in which prompt cessation of anginal attacks followed. Brüning [8] described another successful case, in which after the operation he was able to observe that the blood-pressure differed in the two arms, for it was 20 mm. less in the left arm, to which the sympathetic pathway was interrupted, than in the right arm, to which it was intact. Because of this observation, Brüning, perhaps unfortunately, suggested that removal of portions of the sympathetic might result in a lowering of the general blood-pressure. The records of Jonnescu and Brüning are strong evidence, however, that removal of part of the left sympathetic chain, including the stellate ganglion, causes anginal attacks to abate, though the removal does not greatly affect the general level of blood-pressure.

(2) Treatment of paroxysmal hypertension.—The second direction in which surgical methods have been successful is in the treatment of paroxysmal, as distinct from continuous, hypertension. In 1927 C. H. Mayo [9] described the condition of a patient in whom there were sudden crises during which the blood-pressure rose from a normal figure of 120 or 130 mm. to one as high as 280 or 320 mm. The patient was found to have a chromaffine tumour, about 6 by 4 cm., behind the left suprarenal gland, and the left suprarenal was twice its normal size. After removal of the tumour, the crises did not recur, and the blood-pressure was never greater than 120 mm. Olivier and Meillère [10] have described three cases of paroxysmal hypertension in which the paroxysms disappeared after suprarenalectomy; in these cases a tumour was not present. Kalk [11] found a patient in whom palpation and massage of a tumour in the region of the right kidney caused a rise of blood-pressure from 125 to 155 mm. This patient also suffered from paroxysmal hypertension, the blood-pressure rising to 300 mm. during an attack, and she too was completely relieved from the attacks after removal of the tumour.

Tumours giving rise to paroxysmal hypertension appear to be medullary tumours of the kind known as pheochromocytomas. Not all such growths cause paroxysmal hypertension, for of 53 cases reviewed by Eisenberg and Wallerstein [12] only 25 showed hypertension, and the records do not make clear in how many of these the hypertension was paroxysmal.

The evidence is, however, strong that when paroxysmal hypertension occurs it can be remedied by removal of a suprarenal tumour or of the suprarenal gland, and the cause of the hypertension is, beyond reasonable doubt, the discharge of adrenaline into the blood-stream.

Continuous hypertension.—We may now turn to the side of the picture on which surgical treatment appears to be less successful, concluding first the consideration of the suprarenals.

The published papers do not recognize the possibility that the pathology of continuous hypertension may be quite different from that of paroxysmal hypertension, and some authors appear to think that the two forms are closely related. Certain evidence suggests that this may not be so. Thus Olivier and Meillère [10] state that while removal of the suprarenals will bring paroxysms of hypertension to an end, it does not lower the basal blood-pressure very much, not, for example, below 170 mm. Peyron [13] has pointed out that while paroxysmal hypertension occurs with medullary tumours, continuous hypertension is often associated with cortical tumours. Eisenberg and Wallerstein [12] are also perplexed by this and quote twelve cases of continuous hypertension in which cortical tumours were present. Nevertheless because of what seems merely a hazy idea that hypertension of any form ought to be reduced by suprarenalectomy, some surgeons undertake partial removal of the glands. De Courcy, De Courcy and Thuss [14] have lately described the treatment of two patients with continuous hypertension by the removal of two-thirds of each suprarenal in a two-stage operation. The blood-pressure of the first patient was 260 mm. before the first operation and was 165 mm. three days after the second operation. No later report is given. The second patient had a blood-pressure of 230 mm. before the first operation, and the final blood-pressure shown on the chart after the second operation was 190 mm. In the absence of a later report it seems impossible to say what was the final effect on the first patient, and the effect on the second patient appears to be very slight.

It is, however, in operations on the nervous system that surgeons have been most venturesome in attempting to treat continuous hypertension. As already mentioned, Brüning [8] found that after removal of the stellate ganglion for the treatment of angina, the blood-pressure in the arm of the operated side was 20 mm. less than in the other arm. Royle and Hunter advocated the section of the grey rami communicantes in patients with spasticity, and Royle [15] found the operated side to be warmer. The increased warmth indicated vaso-dilatation. Brown and Adson [16] confirmed the increased loss of heat after bilateral section of the rami and removal of the second, third, and fourth lumbar sympathetic ganglia in five patients with spastic paraplegia.

The evidence indicated that extensive interruption of the sympathetic pathway certainly led to vaso-dilatation in the parts supplied, and therefore suggested that removal of sympathetic ganglia might reduce the blood-pressure in patients suffering from continuous hypertension.

Rowntree and Adson [17] described the result of applying this treatment. They chose a patient in whom medical measures had failed to bring about improvement, whose blood-pressure was 255 mm. The sympathetic ganglia of the second, third, and fourth lumbar segments were then removed on both sides, together with the rami, branches, and adjacent portions of chain. The blood-pressure level was lowered during the first two weeks after the operation and the patient was improved by a disappearance of headache and in his eyesight. Six months later, however, the blood-pressure was again 220 mm., and Adson has stated that the final benefit to the patient was "not significant." This failure did not permanently discourage Adson, for this year, jointly with Brown [18], he has described a still more drastic surgical treatment of hypertension in which he performed bilateral section of the anterior nerve-roots from the sixth thoracic to the second lumbar inclusive. The

main purpose of the operation was again to remove a large portion of the body from sympathetic control. Before the operation the blood-pressure was 180 to 230 mm., and afterwards it was 155 to 195 mm. The latest figures were 160 to 170 mm. Thus the operation produced a fall in systolic pressure of 60 mm., but it is difficult to accept this as due to the removal of sympathetic control. The authors admit that the fall may have been caused by the paralysis of the abdominal muscles which resulted from section of the motor nerves. This suggestion may have much truth in it, for we know that the well-fed rabbit living in a cage will sometimes become unconscious when held up by the ears, because of the lack of tone in its abdominal muscles.

The normal variation in blood-pressure.—The work which has been described leaves the impression that current ideas of vascular control have become fixed and stereotyped, and are in need of reconsideration. These ideas may be said to have been put to the test of experiment, and the results surely indicate that they are wrong, or at least so incomplete as to be a useless foundation on which to build treatment. It is right for a student of pharmacology to begin a reconsideration by discussing the normal limits of physiological variation.

Physiological variation seems as yet to be insufficiently appreciated in characteristics which are not obvious to the eye. The average height of man is about 5 ft. 6 in., but we do not consider heights of 5 ft. or of 6 ft. to be pathological. On the other hand we are inclined to expect that the pulse-rate of any person should be about 72 per minute when he is at rest, forgetting that this is only the average resting rate. Similarly we expect that the normal blood-pressure should be about 125 mm. Now, variations in blood-pressure have been extensively examined, notably by Alvarez [19] who in 1923 published records of 6,000 university men between the ages of 16 and 40, all believed to be in good health. Of the total, about one-fifth were 18 years old, and the figures enable a comparison to be made between the blood-pressure of persons of that age and of persons up to 40 years of age. Alvarez found a continuous variation, in the 6,000 people examined, from 85 mm. to 190 mm. (*See* following table.)

TABLE I.

Pressure mm.	Age 18 years	Age 16-40 years
85-89	3	5
90-94	2	18
95-99	3	38
100-104	14	89
105-109	17	122
110-114	79	447
115-119	116	622
120-124	249	1,196
125-129	124	650
130-134	232	1,104
135-139	100	464
140-144	122	605
145-149	44	179
150-154	57	218
155-159	20	77
160-164	22	98
165-169	4	29
170-174	5	23
175-179	1	10
180-184	1	7
185-189		2
190-194		1
	1,216	6,000
Mean	130	128·9
Standard deviation	13·4	13·5

To those who think of a normal pressure as 125 to 130 mm., figures so extreme as these may seem pathological, but the fact that the variation was continuous, as shown when the figures are plotted as a frequency distribution (fig. 1), suggests that both the very low figures and the very high ones are not pathological, but are normal. The distribution of figures is in every way similar to the distribution of figures for body-height. Further evidence of the probability that the figures are not pathological is obtained from a consideration of the single age-group of 18 years, where the variation was almost exactly the same; the lowest pressure was 85 mm. and the highest 180 mm. When the same variation in blood-pressure is found in 1,200 people of 18 years of age as in 6,000 people covering the ages 16 to 40, it is likely that the same explanation covers both groups, namely, that the variation is physiological.

Alvarez found that the mean pressure was 130 mm. in the 18-year group and 128·9 mm. in all age-groups. The standard deviations of the two means were 13·4

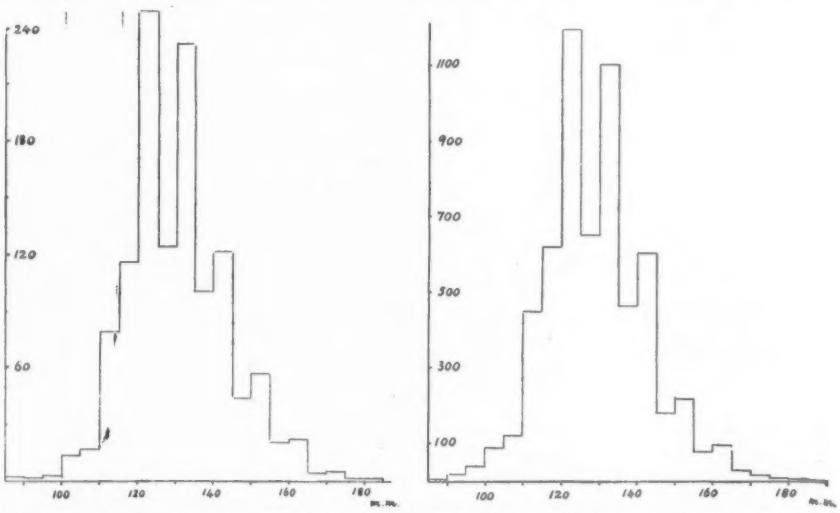

FIG. 1.—Showing frequency-distribution of the normal blood-pressure (from Alvarez's tables). Blood-pressure as abscissa, number of persons as ordinate. On the left is shown the distribution of figures for 1,216 persons of 18 years of age. On the right are the figures for 6,000 persons from 16-40 years of age.

and 13·5 respectively. Now much can be learned from the standard deviation. Taking for convenience the mean or average pressure as 130 mm. and the standard deviation as 13·5, we learn that if we examine any three healthy people, two will have a pressure between 116·5 mm. and 143·5 mm., but the third will have a pressure less than 116·5 mm. or greater than 143·5 mm. That is to say, one-third of all males between 16 and 40 years of age will have a pressure less than 116·5 mm. or greater than 143·5 mm., and since the distribution is roughly symmetrical, about one-sixth of these males will have a pressure greater than 143·5 mm. Alvarez actually found 22% with a pressure over 140 mm.

Again the standard deviation tells us that if we examine any 22 healthy people, one of them will have a pressure either less than 103 mm. or greater than 157 mm. So that one man in about every 44 men must have a pressure greater than 157 mm., a pressure which is just as normal as the colour of his eyes. Alvarez found a pressure greater than 160 mm. in one man in 36.

Clearly the ordinary teaching that a normal blood-pressure should be 125 or 130 mm. is dangerously misleading. How many doctors realize that a blood-pressure greater than 160 mm. will be observed in one in every 40 healthy men? How many unfortunate people are going about under the impression that they are suffering from high blood-pressure, and making vain efforts to reduce it, when, in fact, their pressure merely shows an unusual deviation from the mean?

Finally, one may ask, does the disease known as essential hypertension really exist, or is it not a figment of an uninstructed imagination? The possession of a high blood-pressure admittedly exposes a man to dangers from which others are freer, just as unusually tall people have to take care not to knock their heads, but apart from this greater risk, one may doubt whether a high blood-pressure should be classified as a disease, unless there is evidence that the pressure is continuing to rise. The observation of a pressure of say 170 mm. does not justify a diagnosis of hypertension unless the doctor knows that at a previous examination the pressure was much lower.

The depressor action of adrenaline.—The agents which cause vaso-constriction, namely the sympathetic nerves and the hormones adrenaline and vasopressin, are

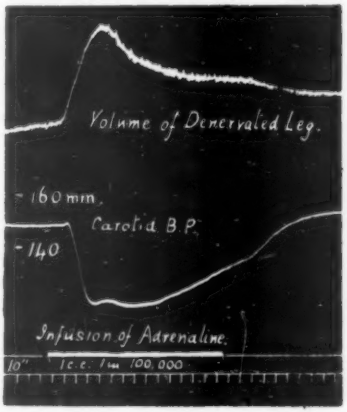

FIG. 2.—Showing depressor and vaso-dilator action of adrenaline (Burn and Dale).

now known to be able to cause vaso-dilatation in certain circumstances, though hitherto it is not clear how far these circumstances may arise in the body. Nevertheless, these vaso-dilator effects must be borne in mind when arriving at an estimate of the causes of high blood-pressure. Moore and Purinton [20] first showed that small doses of adrenaline have a depressor effect when injected intravenously into the cat anæsthetized with ether. This depression of the blood-pressure is a true vaso-dilatation produced by a peripheral action, since it is accompanied by expansion of the volume of a limb the nerves to which have been cut. It is most easily seen when the blood-pressure is high, when, for example, a solution of adrenaline is allowed to run into a vein (fig. 2). It is possible, or indeed probable, that the dilator action is exerted in all circumstances, though usually masked by the accompanying constrictor action. The action is worth mentioning here to remind us that adrenaline is not a purely constrictor substance, and that its dilator effect may be of importance in precisely those conditions of high blood-pressure which some seek to remedy by partial removal of the suprarenal glands. Rolleston [21] says that, "In Addison's

disease" (in which, of course, there is suprarenal deficiency) "a low systolic pressure, even to 50 mm. Hg. is essential for diagnosis." There is, however, some doubt of this, for Packard and Wechsler [22] state that "typical Addison's disease, in patients with co-existing essential hypertension, may occasionally be associated with high blood-pressure." Further, it is easy to forget that the customary low blood-pressures of Addison's disease do not necessarily indicate dilatation of the vessels, but are as likely to be due to the weakness of the cardiac muscle.

The dilator action of pituitary extract.—If we turn to extract of the posterior lobe of the pituitary we have a second hormone which as its name, vasopressin, implies, is commonly regarded as vaso-constrictor.

Harvey Cushing [23] has, however, recently described the effect of administering it not into a vein, but into the lateral ventricle of the brain. He made observations on patients in whom portions of the roof of the skull had been removed for the exsection of cranial tumours. When doses of 20 units of pituitary extract were injected into the lateral ventricle he observed, not the sickly pallor which follows intramuscular injection, but an intense flushing of the skin and sweating. This flush was absent from the skin covering the opening through the bone, and since the nerves to this portion of skin had been removed, the flushing must have been due to a central action.

The vaso-dilatation and sweating were attended by other effects, namely, salivation and vomiting, and a fall of body temperature and basal metabolism. In two hours after the injection, the body temperature fell in one patient to 93°, rising again to 97° in two hours more. What has interested Cushing so much has been to note the resemblance between these effects produced by pituitary extract and effects produced by intraventricular administration of pilocarpine. Pilocarpine also causes flushing, sweating, vomiting and a fall of body temperature, but these effects are produced by intramuscular as well as by intraventricular injection. Cushing argues that pilocarpine produces its effects by stimulating the terminations of the parasympathetic system of nerves and, since it can act by intraventricular injection, considers that its central effect is upon a parasympathetic centre in the brain. He urges further that the similar action of intraventricular pituitary extract indicates that it, too, stimulates a parasympathetic centre. This is an important suggestion.

It would be foreign to the main purpose of this discussion to consider in detail how far Cushing's explanation of his observations is correct. Acetyl-choline rather than pilocarpine is the chemical substance which acts like the parasympathetic system, and in some respects the vaso-dilator action of pilocarpine is nearer to that of histamine than to that of acetyl-choline (Burn, [24]). Until a year or two ago it was perhaps possible to doubt as Cushing does, whether sweating was really a sympathetic effect, but the recent researches of Dale and Feldberg [25] leave no room for this possibility. Sweating is caused by the sympathetic nerves, and sweating due to the administration of pituitary extract into the ventricle must occur because of a discharge of impulses in sympathetic, and not in parasympathetic, nerves.

For the present argument, the important fact emerging from Cushing's observations is that when pituitary extract is injected in large dose into the ventricle, which may for all we know be the normal means of entry of the hormone into the system, it causes vaso-dilatation and not vaso-constriction, from which it follows that pituitary extract, like adrenaline, may play a part normally in keeping the blood-pressure low.

The sympathetic vaso-dilators.—In an account of some work given to this Section a year ago, I described (Burn [26]) how it is easily possible to observe that stimulation of the lumbar sympathetic chain in the dog causes a dilatation of the vessels of the two hind limbs. The observation is made when the hind limbs are perfused with

defibrinated blood by means of a Dale-Schuster pump [27] and when adrenaline is added to the blood so as to maintain a steady, though not high, tone in the vessels.

Two new points may be mentioned. First this dilatation is not seen in the vessels of the cat, and second, it is evident after the removal of the skin from the limbs of the dog, so that it must take place in the muscles (fig. 3). In fact I have not yet observed constriction to follow sympathetic stimulation after skinning the limbs, though it is too soon to say that it does not occur.

The absence of a vaso-dilator effect in the cat, and its presence in the dog, may be connected with the much greater power possessed by the dog for prolonged muscular work. It may be that in the dog and in man the sympathetic nerves are constrictor to the vessels of the skin and of the intestines but dilator to the vessels of the skeletal muscles. The conception of a constrictor supply to the skeletal muscles has always seemed to me difficult to understand. The immediate purpose of general sympathetic stimulation in times of stress must be to secure an efficient

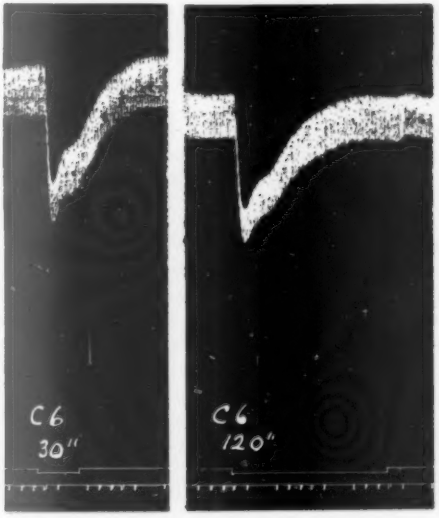

FIG. 3.—The vaso-dilator effect of sympathetic stimulation in the perfused hind limbs of the dog after removal of the skin.

blood-supply to the heart by raising the blood-pressure and so improving the coronary circulation. This can be achieved by cutting off the blood to the skin and the intestines. But a free channel for blood to the muscles is essential if the body is to be able to fight or to run away, and if the vessels of the muscles were constricted also, the change would surely defeat its own object.

In suggesting that the sympathetic supply to the muscles is mainly dilator, Michell's [28] observations on Cambridge rowing men may be recalled. Their average blood-pressure was found to be "when just awakened and lying in bed, 95 to 100 mm. Hg; after rising and while moving about the room it is 100 to 115; two hours after hard exercise and immediately on lying down 130 to 125; from this highest point the fall becomes rapid, so that after the man has been lying down for ten minutes it is often 106 to 110."

If in those accustomed to severe and prolonged muscular exertion the sympathetic dilator nerves attain a higher degree of activity than in other persons, it is conceivable that the blood-pressure of these persons would be lower for this reason.

At present it is too early to reach any conclusion about the precise function of the sympathetic dilator nerves. There can I think be no doubt of their existence, and their existence makes it clear that the sympathetic system is not only concerned to keep the blood-pressure high. The duty of the sympathetic must be to regulate the blood-pressure, to raise it and to lower it according to need, and it should, therefore, be remembered that any operation which removes a portion of the sympathetic removes not only the means of raising the blood-pressure but also the means of lowering it.

Thus for all three mechanisms which are commonly recognized as pressor in function, namely, the circulating hormones adrenaline and vasopressin and the sympathetic nervous system, there is evidence that in certain circumstances they are depressor. Those who are concerned with the treatment of high blood-pressure ought not to assume that the condition they wish to treat is one in which there is overactivity of one of these mechanisms; in fact the hypertension may be due to deficient activity. If this point of view is adopted, heroic operations in which large portions of the sympathetic system, or of the suprarenal glands, are removed, should no longer be attempted.

The action of ether on blood-pressure.—I propose to conclude this paper by discussing some observations which were made a year or two ago by Bhatia and myself [29] relating to the action of ether on the blood-pressure, for a consideration of the effect of an anæsthetic so widely used is, I hope, of some general interest. The current teaching is that ether has little effect on the heart. Respecting the vessels, one well-known textbook [30] says: "Ether seems to have little or no direct action on the vaso-constrictor centre, but the dilatation of the skin vessels indicates that it excites the vaso-dilator function directly or indirectly. The direct action on the vessel walls seems to be of greater importance than that on the innervating centres." A second book [31] says: "Ether has a less depressant action on the medullary centres than chloroform." These statements, that there is little or no central action, and that what central action there may be is depressant, scarcely accord with the common laboratory experience that the blood-pressure in a cat under ether when the vagi are cut is usually very high; I have often seen a blood-pressure of 250 mm. To what is the high pressure due? If there is no constrictor effect on the vessels, or if, indeed, the vessels are dilated, the rise of blood-pressure must be caused by an increased output from the heart. To test this possibility we examined the effect of ether on the heart by using the heart-lung preparation of the dog. In this preparation, as is well known, the blood leaves the aorta to pass through an artificial peripheral resistance to a reservoir, from which it returns to the vena cava. The only organ in circuit besides the heart is the lungs. When ether vapour was added to the air with which the lungs were inflated there was an immediate expansion of the cardiac volume, indicating a weakening of the power of the individual fibres and a fall in the pressure with which the blood was expelled (*see* fig. 4). We were surprised at the extent of the dilatation caused by a small amount of ether, and accordingly made a careful quantitative comparison with the effect of chloroform. We found that the dilator effect of chloroform was indeed much greater than that of ether, as is commonly taught, but we believe that too much emphasis has been laid on this difference between ether and chloroform and that it is not sufficiently realized how much ether itself weakens the heart. How then is the high blood-pressure in a cat under ether to be explained if ether weakens the heart? The answer may be that it is due to a direct action on the vessels, causing them to constrict. This possibility was tested

by examining the action of ether on the tone of the vessels of the hind limbs of a dog perfused with blood by a Dale-Schuster [27] pump. The result (*see* fig. 5) showed that ether had no appreciable action on the vessel walls themselves. The inference seemed obvious that when ether is administered to an animal there must be increased vaso-constrictor tone because of impulses passing along the sympathetic nerves. Now impulses of this kind can be cut out by the administration of nicotine,

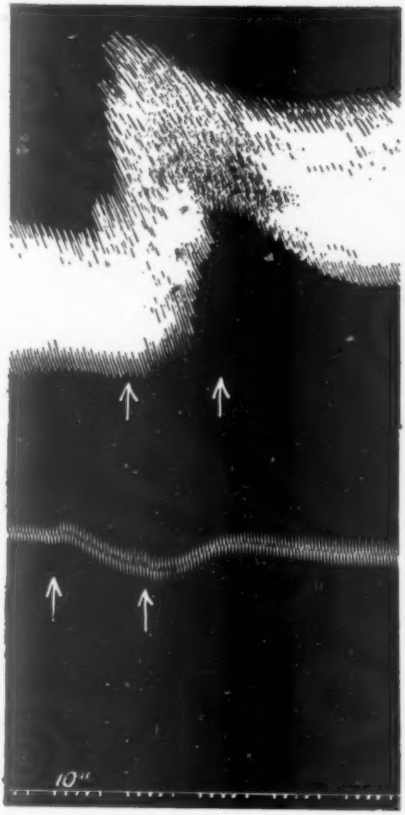

FIG. 4.—The effect of ether on the heart of a dog in the heart-lung preparation showing the expansion of the cardiac volume and the fall in the pressure of the blood expelled from the aorta. Top, cardiac volume; bottom, arterial pressure.

which in large dose paralyses the sympathetic ganglia so that no further stimuli are transmitted by them. We therefore examined the effect of ether on the blood-pressure in decerebrate cats, before and after the administration of nicotine. Although the cats breathed spontaneously, artificial respiration was given, and at intervals ether vapour was mixed with the air to observe its effect. Fig. 6 shows an experiment in which, before the administration of nicotine, ether had almost no effect on the blood-pressure, whereas after nicotine ether caused a steep fall. The

fall seems best explained as the uncomplicated effect of the weakening of the heart on the blood-pressure. The fall presumably was absent before nicotine was given because vaso-constriction compensated for the weakening of the heart. In support of this is the evidence from the same figure which shows that ether administration causes a contraction of the spleen, which is an organ having a motor sympathetic innervation; this contraction was not seen after nicotine. Similarly, it was found that ether caused prompt inhibition of intestinal movements. The effect on the spleen being motor can only be interpreted as due to stimulation by way of the sympathetic nerves. Whereas vaso-constriction might be taken to be a reflex effect caused by the diminished cardiac output, the effects on the spleen and the intestines make it probable that the impulses passing down the sympathetic nerves are not reflex but due to direct central stimulation. Evidence that ether weakens the heart

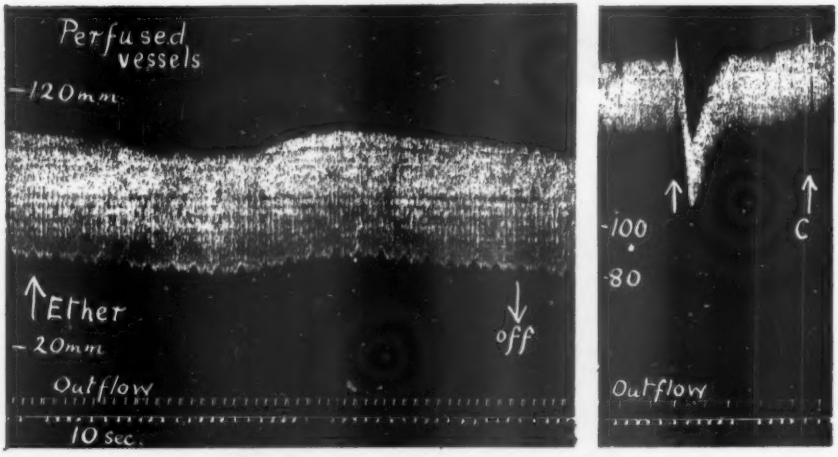

FIG. 5.—Showing that ether has no appreciable direct action on the vessel-walls of the hind limbs of the dog perfused with defibrinated blood. The tracing records the peripheral resistance in the vessels. On the right is shown the effect of an intra-arterial injection of saline saturated with ether.

and constricts the blood-vessels has also been obtained by Cattell [32] using quite different methods, so that the conclusion may be drawn with some confidence that during ether-anæsthesia the heart is greatly weakened, but the vessels are actively constricted. It is worth while to consider the bearing of this conclusion on the use of ether as an anæsthetic. It may be pointed out in the first place that if ether had not the action on the vessels which has been described it could not be used as an anæsthetic at all. For if the vessels did not constrict when the cardiac output fell, the reduction in blood-pressure would lead to a reduction in coronary flow, and because of this the weakening of the heart would be intensified; the patient would become moribund in a few minutes. But the arteries constrict so that the blood-pressure does not greatly fall, and in consequence the coronary flow remains high. Nevertheless, although throughout prolonged anæsthesia the heart-beat is well maintained, the heart is delivering blood through constricted arteries, and it follows that the supply of blood to the periphery must be much smaller than usual. There must be a deficient peripheral circulation with resultant anoxæmia, and it may be this which is the cause of the greatly increased sensitiveness to histamine which was

demonstrated by Dale [33]. A weakened heart and a deficient peripheral circulation cannot fail to do harm to the patient, and a realization that these effects are produced should make those hesitate who still regard ether as the safest of general

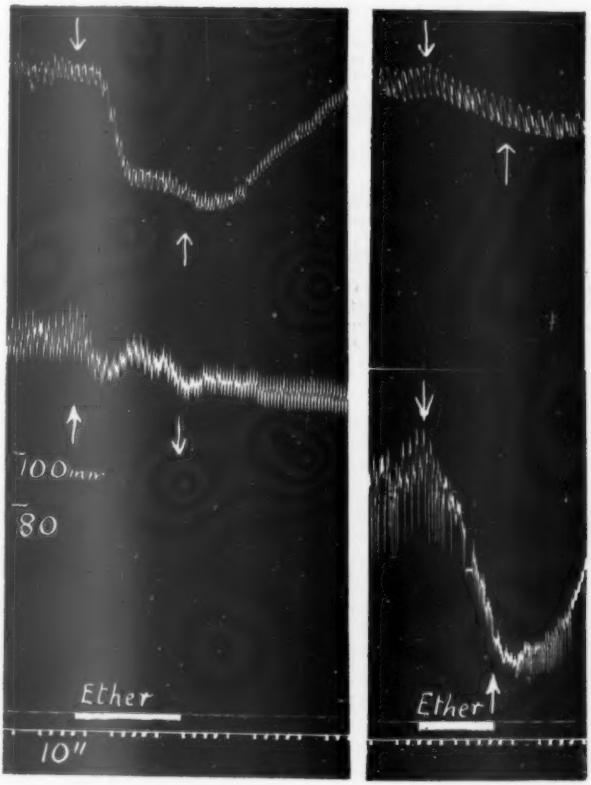

FIG. 6.—Showing that in a cat from which the suprarenals have been removed ether produces a contraction of the spleen volume before nicotine is administered, but not afterwards; similarly ether has little effect on the blood-pressure before nicotine, but causes a large fall afterwards.
Top, spleen volume.
Bottom, carotid blood-pressure.

anæsthetics. The available evidence indicates that ether produces serious circulatory disturbances, and a recognition of what these disturbances are should encourage the use of some of the newer anæsthetics.

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Section of Ophthalmology

President—RANSOM PICKARD, C.B., M.S.

[*October* 12, 1934]

The Causation of Herpes Ophthalmicus

PRESIDENT'S ADDRESS

By RANSOM PICKARD, C.B., M.S.

THE subject of this paper is the causation of herpes zoster and the reason of the special manifestations in and about the eye when the first division of the fifth nerve is affected. No apology need be made for dealing with a portion of general medicine in a specialist Section of this Society. We all recognize the need of keeping in touch with general medicine and of studying the reactions of the body and its parts upon each other. But, seeing that this subject was discussed as recently as 1924, at the Annual Congress of the Ophthalmological Society of the United Kingdom, a reason for reopening the subject should be given. It is to dwell upon an aspect which was not then, to my mind, fully considered that this paper is written.

In dealing with herpes zoster, where distinct proof of its causation is lacking, we are forced back upon clinical manifestations and a comparison with any analogous disease which exists, the cause of which is known, at least in part.

The main clinical fact established in late years is the relationship of herpes zoster and varicella. The present position of this aspect of herpes zoster was fully and clearly dealt with by Mr. L. H. Savin in 1933 [1]. Either disease may cause the other; less frequently they may exist in the same person at the same time. Obviously, the causal organism has two phases—one generalized, causing varicella, the other localized, giving rise to herpes zoster.

That these two phases are unstable is shown by the moderate frequency with which the two diseases are transmuted in passing from one person to another, but the varicella phase is the more infectious; as such, it more often breeds true.

The analogy (not identity) with herpes corneæ is obvious in that the latter has two phases, dermatotropic and neurotropic. The analogy further extends to this, that there is to some extent an interchangeability between the two strains of herpes corneæ, though the encephalitic variety becomes more specialized and less transmutable than the dermatotropic. Here the analogy ceases, for both phases in herpes corneæ are local in position, inasmuch as the general nerve symptoms in the encephalitic variety, being due to the localization in the brain, are not general in the sense that varicella is general.

The pathology of herpes zoster has been well investigated by Head and Campbell [2]. They showed that the main seat of inflammation was in the posterior root ganglion, and that it spread up and down the nerve to a slight extent. Degeneration sometimes spread up into the posterior column of the cord, and down the sensory

nerve and its branches, to their entry into the cutis. The signs of inflammation in the posterior-root ganglion did not involve the whole of it. In Cases 20 and 21 (loc. cit.) no lesions of the ganglia and nerves were found eight months after the attack. The effect of a severe attack of herpes ophthalmicus upon the eye was very fully dealt with by J. Meller in 1920 [3]. The eye was severely affected with iritis and hypopyon, and it was finally excised. The iris was in part necrosed below, and the adjoining portions of the ciliary body and sclerotic were involved. In the uvea near to this, in front of the ora serrata, was a small nodule. The retina was unaffected except by infiltration of a few mononuclear cells. The optic papilla was slightly œdematous with a few round cells in it. The optic nerve showed destruction and fibrillation of the medullary sheaths of the nerve-fibres.

The ciliary nerves showed marked changes. They were surrounded by a sheath of mononuclear cells, the endoneurium also having many of these. The medullary sheaths did not stain. On the other hand, the ciliary vessels were not affected. While the ciliary nerves supplying the lower half of the eye were much inflamed, those of the upper half were nearly normal. The general picture is that of a very severe but localized affection of the iris and adjoining structures; in the necrosis of part of the iris a resemblance to the sloughs which form in the skin is obvious.

The explanation that the skin eruption in herpes zoster is due to an alteration in the trophic impulses from the affected posterior root ganglion or spinal cord is still generally held; it is here suggested that this theory is quite inadequate to explain the groups of vesicles in the skin or the various manifestations in and about the eye.

Levaditi [4] has a variant of the trophic impulse theory. He believes that there is a normal "tonus immunotrophique" in each spinal segment which prevents infection; the lowering of this enables the organism to establish itself in the segment and that part of the body supplied by it.

Head and Campbell (loc. cit. p. 395) reject the production of the rash by the disturbance of special trophic nerves, but suggest that it is caused "by an intense irritation of the cells in the ganglion which normally subserve the function of pain, and more particularly that form of pain produced by afferent visceral impulses."

It being granted that, normally, influences pass down the nerve-fibres which have for their object the maintenance of the tissues in a healthy condition, let the possible variations of these stimuli be considered. Their complete absence is given when a nerve containing sensory fibres is divided: the resultant atrophic changes are considered as being due to the absence of these trophic impulses. It is obvious that herpes zoster is not due to this absence. Can it be explained by an increase in the normal impulses, or does the inflamed ganglion produce an abnormal form of impulse resulting in herpes zoster?

If the rash were due to increased activity in the posterior root ganglion, causing an increase in the normal trophic impulses, it would be expected to produce a more uniform effect on the skin than actually happens. For the lesions in the ganglion are usually diffuse where found, not limited to minute areas such as might correspond to the individual vesicles.

Ruling out an increase in normal impulses, let the possibility of an abnormal impulse be considered.

Taking a case of herpes zoster sufficiently severe to cause vesicles to appear in all parts of the skin area corresponding to the ganglion affected, it will frequently be found that the skin is not even red in the areas between the individual vesicles. Thus a selectivity is shown, so that the groups of vesicles are separated from one another and within the group the individual vesicles do not always coalesce. It is fair to conclude that the vesicles are caused by the exciting organism itself, acting either from a distance by nerve impulses, by the transmission of toxins along the nerves, or by the actual passage of the organism. Toxins may be excluded; a substance of molecular or colloid dimensions would be distributed in a much more

even manner than is the skin eruption in herpes zoster. The fundamental objection to the skin eruption being caused by nerve impulses is that the eruption is essentially the same as in varicella, so far as the individual vesicles are concerned. The assumption that the posterior ganglion phase can call up the skin in the area of the ganglion, inducing it to act as if varicella were present in the skin, is unsupported by experimental proof or by analogy from any other disease processes. If there were other diseases one phase of which could call up a resemblance to the clinical symptoms of the other phase, and in which it had been definitely proved that this initiation was due to nerve impulses, then the argument by analogy would be admissible. But this is not so; it remains an improbable assumption.

The simplest explanation of any phenomenon, consistent with the known facts, should be accepted. If the theory does not account for all the facts, it may be temporarily accepted and used as a basis for investigation, the inconsistent facts being always borne in mind.

It may be regarded as proved by pathology that the herpes-zoster phase has a special affinity for the posterior root ganglia and that one of these is its primary seat, Let it now be assumed that normally it spreads thence along the sensory nerves to the tissues supplied by them, and that, on reaching fibrous tissue covered by cells of ectodermal origin, the organism reverts to the varicella phase. Let it also be assumed that in some cases the organism may spread locally from the posterior ganglion, thus reaching adjoining structures.

Now apply these assumptions to the case of herpes ophthalmicus; the effects of this in and about the eye are so numerous that they form the best clinical test of any theory of causation of this disease. The skin eruption is explained by the reversion of the posterior ganglion phase to varicella on reaching the cutis, which is covered by epithelium. The usually greater density of the eruption, as compared with varicella, occurs because the growth of the organism is concentrated along the nerve, while varicella is diffused by the blood-stream. The cornea is covered by epithelium, therefore its affections are similar to the skin; it being remembered that as herpes zoster does not always ulcerate or become vesicular in the skin, neither does it in the cornea. The conjunctiva and the part of the sclerotic covered by it conform to the rule laid down. The iris and ciliary body—covered by cells derived from the primitive optic vesicle and so remotely of ectodermal origin—behave similarly. But the retinal epithelial cells do not bound a surface and are very highly specialized, therefore affections of the sclerotic behind the ora serrata of the choroid, and of the retina are exceedingly rare. Head [5, p. 481] in 1910 wrote that in 378 cases of herpes ophthalmicus he had never seen changes in the retina or optic nerve.

Wilbrand and Soenger [6] mention a case of Wangler's in which, with no inflammation of the cornea and iris, there was optic neuritis and an inflammatory mass in "der Ader-oder Netzhaut."

Haltenhoff in 1893 [7] described a case in which there was retinal venous thrombosis in an affected eye, with later slight optic atrophy. He discusses whether this may be due to sepsis spreading from the skin eruption to the veins of the orbit; it cannot be considered as a primary affection of the retina, since the thrombosis was in the nerve head. He refers to a similar case reported by Oscar Wyss [8].

The *Transactions of the Ophthalmological Society of the United Kingdom* for the last thirty years contain no case of retinitis or choroiditis associated with herpes ophthalmicus. Thus, in the library of the Royal Society of Medicine there have been found accounts of two cases of thrombosis of the retinal vein, and one case in which, associated with optic neuritis, a nodule of inflammatory tissue was found "in the choroid or retina." Meller's case [3] does not come into this group, for the nodule in the uvea was in front of the ora serrata, as the illustration in the article shows; it conforms to the theory stated in the present paper.

Head [5, p. 491], in dealing with the general problem of herpes zoster, states there is "no evidence that the deep organs are affected." Nor should they be, according to the supposition made here; for cavities such as the pleura, not having surfaces covered by epithelium, would not give a chance for the neurotropic phase to turn into the dermatotropic phase.

The time-factor may now be considered. The usual sequence is that pain occurs three or four days before the rash appears; the interval represents the growth of the organism along the nerves. More difficult to explain is the late occurrence of keratitis, iritis, and cyclitis, which is so frequent that we are accustomed to speak very guardedly in the first two weeks or so as to the escape of these structures. It may be that there are anatomical reasons for the delay, or in this renewed attack of the organism there may be a resemblance to the successive crops of vesicles which are so characteristic of varicella.

Let the lesions other than those of the skin and globe be now considered. Take a rare complication of herpes ophthalmicus, affection of the optic nerve with complete and permanent blindness. There is no anatomical or developmental connexion between the optic nerve and the Gasserian ganglion, but they are 2 to 3 mm. from each other. Such a case is most simply explained by local spread to the optic nerve. A similar reasoning will apply to paralyses or pareses of the third, fourth and sixth nerves, which are still closer to the Gasserian ganglion, without invoking the fact that it is the posterior root ganglion for these nerves.

One special feature of herpes zoster, especially in the first division of the fifth nerve, is its erratic distribution and intensity. A marked example of this was a case in which, with only one group of vesicles, on the outer half of the eyebrow and adjoining skin, acute glaucoma co-existed. If a nerve is thought of as branching like a tree, along which an organism is propagating and spreading, it will be recognized that it is at least inherently probable that it will not spread along all branches equally, and will miss some completely, especially if it happens that the organism is either small in quantity or of feeble growth. This affords an explanation of the great variation in the symptoms of herpes zoster.

It has been suggested that it is quite possible that the variation which results in varicella or herpes zoster may reside in the skin and posterior ganglia, the virus being constant, not two-phased. Variation in the causal organism has been proved in various diseases, e.g. the varying strains of pneumococci and streptococci and, as more apposite to the present case, the dermatotropic and neurotropic varieties of herpes corneæ. So that the assumption of a two-phased virus is correlated to our present knowledge. But the clinical picture is not completely accounted for by the assumption of a one-phase organism, for herpes zoster occasionally breeds true. Epidemics arise unaccompanied by varicella; if the same virus gave rise to both, this would be a most improbable occurrence. It is common to both assumptions that the skin is not immune to varicella, for in both the one and two phase theory it is assumed that the herpes zoster rash is varicella. If it be assumed on the one-phase theory that, whatever its port of entrance into the body, the virus enters the blood and so reaches the skin and ganglia; then, because of the greater bulk of the skin as compared with all posterior root ganglia taken together, the chance of any person having herpes zoster without varicella becomes very remote, the possibility of an epidemic of herpes zoster without varicella much more unlikely. The picture of a posterior root ganglion successfully competing with the skin for the possession of the whole of the virus present, and the skin being so easily induced to receive the virus shortly afterwards is difficult to imagine. It involves an inconsistency which is avoided by the two-phase theory. The conception of a two-phase virus, each easily transmutable to the other, will account in a simple manner for the epidemics of herpes zoster and varicella, each breeding true, for the interchange which occurs fairly often, and for the occasional occurrence of both in the same individual.

So far only herpes zoster which exists as an entity and is related to varicella has has been dealt with, an important consideration is that it may occur in other diseases and in poisoning from various causes. Levaditi [4, p. 32] suggests that these are really cases of ordinary herpes zoster, occurring because the lowered resistance of the nervous system allows the organism to take up its abode. But there is the possible alternative that more than one organism can behave like that which causes ordinary herpes zoster. The matter cannot be determined, but some cases have occurred in series, suggesting infection.

That the virus of rabies spreads along the nerves centripetally to the central nervous system has been proved. Similarly the toxin of tetanus reaches the central nervous system along the nerves. But it has not been proved that a virus can travel centrifugally, as has been assumed in the present paper. There are two forces which would act against it. Of these the electric currents which occur when sensory impulses pass along the nerves are so weak that they can scarcely be regarded as having a lethal or paralysing effect on an organism. The lymph stream would be a more potent and stable factor, but this must be excessively small.

The final test of this theory must be experimental, but there are great difficulties in the way. Initially a susceptible animal must be found. Of course, the material from a herpetic vesicle or the underlying skin must be used; but, if the theory is correct, the vesical is a reversion to the dermatotropic phase. In that case there might be a great difficulty in inducing the organism to return to the neurotropic phase, though obviously the contents of the herpetic vesicle would be more likely to succeed than those of a varicella vesicle because of their recent derivation from the posterior ganglion phase. Lastly, the inoculation must succeed both in the posterior ganglion and the nerve trunk before the theory could be regarded as proved.

Should the suggestion given in this paper be correct, it would explain many of the failures in experimental work on herpes zoster. No inoculation on the cornea, for instance, would be likely to succeed. The work would have to be founded on Nature's method of producing this disease; even so, failures would be frequent because of the difficulty in getting the dermatotropic phase to revert to the neurotropic phase. And any work which results in encephalitis without herpes zoster must be excluded.

In conclusion, the thesis of this paper is that the virus causing varicella and herpes zoster has two phases, easily transmutable. The herpes zoster phase has its primary seat in a posterior root ganglion. It may spread locally, involving adjoining nerves in the case of the Gasserian ganglion; but its main and normal extension is along the sensory nerves to fibrous tissue covered by epithelium, where it reverts to the varicella phase. It there produces nodules, usually with vesication, sometimes accompanied by ulceration and even sloughing. The affections of the eye in herpes ophthalmicus are explained by this theory, as are those epidemics of herpes zoster unaccompanied by varicella.

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Retinal Detachment in an Aphakic Eye, cured by Safar's Method.—T. COLLYER SUMMERS, F.R.C.S.

A. F., female, aged 62. This patient was first seen on July 2, 1934, complaining of sudden loss of vision of the left eye two weeks previously. A large billowing detachment was seen above, affecting more than half the retina. The eye was

aphakic, the lens having been removed in July 1933. A small round hole was found at about 10 o'clock and about four discs diameter from the ora. The field of vision to a 2 mm. white object was reduced to a small area in the upper temporal field (fig. 1). Vision in the eye was less than $\frac{6}{60}$. The right eye had perception of light only, following a cataract extraction two years before.

Safar's operation was performed on July 10, 1934. (Forty-six punctures with a maximum amperage of 70 milliamps.) An uninterrupted recovery took place, the vision being $\frac{6}{24}$ on August 16, 1934, with the proper correction. On October 1, 1934, vision with + 9 sph. and 0·5 cyl. was $\frac{6}{9}$ partly.

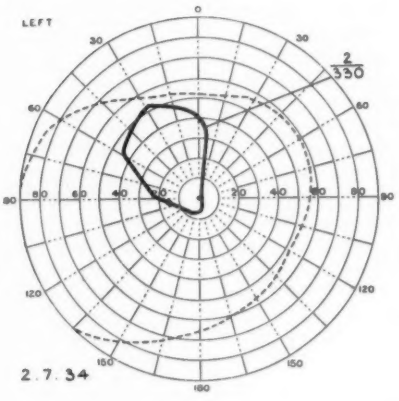

FIG. 1.

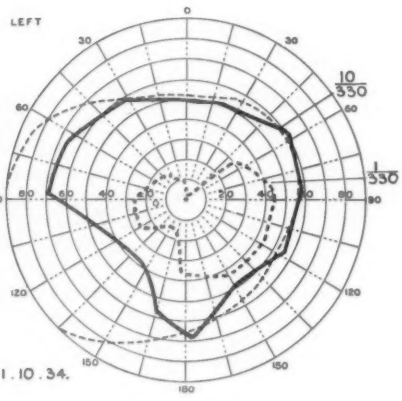

FIG. 2.

The whole retina is still in situ and a large mass of old choroiditis marks the site of operation. There is now only the defect corresponding to the area destroyed by operation found in the 10 mm. white field, but the 1 mm. white is reduced very considerably, particularly where the original field existed, i.e. the upper temporal portion (fig. 2).

I have shown this case firstly, because it is of interest as being one of complete recovery in an aphakic eye; and secondly, on account of the loss of field to a fine object in that part which was originally the best.

It seems to me that the electrical conductivity of the material under the detached retina is not so good as that round the globe itself where the retina is attached, and that more damage is done by the current to the part of the retina which is attached than to the rest. This, I think, constitutes a plea for the more accurate localization of holes than is usually the case at present.

Multiple Congenital Deformities.—E. F. KING, F.R.C.S.

F. P., male, aged 16. Vision in both eyes always defective.

Right eye: Nystagmus. Polypoid masses, covered by conjunctiva, in upper fornix. Multiple dermoids, with local opacities of cornea at limbus. Malformation of optic disc. Vision $\frac{6}{24}$ partly with + 2·5 cyl. ↘ 150 = $\frac{6}{24}$.

Left eye: Defect in lid at junction of inner and middle thirds. Nystagmus. Polypoid masses, similar to those in right eye, though more marked, in upper fornix. Whole of cornea covered by dermoids. No view of fundus. Vision: Counting fingers at 18 in.; not improved.

General examination shows left side of thorax under-developed and left testis undescended. Abdominal reflexes absent. Skiagrams of skull and thorax reveal no abnormality.

Tumour of the Iris.—A. F. MACCALLAN, F.R.C.S.

This patient came to hospital on September 18, asking to be examined for glasses. She was short-sighted, having 7D myopia in one eye, 10D in the other. Her only complaint was that she could not see. She was found to have a small tumour in the iris of the left eye, in the position of 23 to 28 minutes past on the watch face. I would like to know the nature of this tumour: is it inflammatory in origin; is it a congenital, innocuous condition; is it a benign melanoma; or is it a melanotic sarcoma?

Mr. J. GRAY CLEGG: said that in his opinion this was a congenital condition: he had seen a similar case in which the iris was attached to the cornea, and had the same kind of straight-line, grey appearance as in the present case. He counselled watching only, for some months, before deciding to do anything operative.

Bilateral Proptosis: ? due to Sphenoidal Fissure Syndrome.—EUGENE WOLFF, F.R.C.S. and E. D. MACKWORTH, M.B.

Patient, female, aged 50, had a bad cold two weeks before admission to hospital. One week previously a severe headache with a loud "throbbing" noise in the head commenced, accompanied by vomiting.

State on admission.—Temperature 100° F. Pulse and respiration normal. Left eye markedly proptosed and does not pulsate. Conjunctiva injected. Both lids of left eye reddened and slightly œdematous. Sight unaffected. Eye movements limited in all directions. Left pupil smaller than right; reacts directly and consensually. Fundus normal.

Nose and throat report.—There is well-marked septal deflection to left. No pus in nose. X-ray examination of sinuses negative. A loud blowing systolic murmur is audible all round the left orbit.

The temperature fell to normal three days after admission.

In a fortnight from date of admission the proptosis was less marked, the conjunctiva less injected and the œdema of the lids less marked. The murmur remained. The movements gradually returned but outward movement remains greatly diminished.

After eight weeks it was noted that the proptosis was less and the ocular movements more free, but there was now injection of the right eye with proptosis and limitation of movement. In both eyes it was remarked that the external rectus was most affected. The sphenoidal fissures were examined by X-rays, but did not show any abnormality. At no time was any enlargement of the thyroid gland discerned and the pulse has consistently remained at 80. Blood-pressure 165/90.

Wassermann reaction negative.

Discussion.—Mr. D. V. GIRI asked whether in this case there might not have been a slight ethmoiditis, setting up mild bilateral orbital cellulitis, which had cleared up by the time the skiagrams were taken.

Mr. C. B. V. TAIT said that there were two points against this case being one of periostitis in the sphenoidal fissure. One was the marked proptosis—a condition not met with, as a rule, in periostitis—and the other was the presence of a systolic bruit, which it was difficult to harmonize with the suggestion of periostitis.

Mr. F. A. WILLIAMSON-NOBLE said that when he was pathologist at the Central London Ophthalmic Hospital he had had a similar case. The eye had been removed and the orbit exenterated. The proptosis had occurred after influenza, and he was only able to find a mass of lymphocytes. A tentative diagnosis of lymphoma was made. He looked into a work on pathology and found that lymphomata were not regarded as tumours in the proper sense of the term; it was simply that lymphocytes began to grow and continued to do so. Perhaps if the surgeon in charge had exercised patience and waited, the eye might have returned to its place. In that case there was no record of any bruit.

The PRESIDENT said that the difficult symptom in this case was the bruit. Apart from that, he thought the condition was a definite sphenoiditis spreading to the orbit. He had seen the condition in such cases clear up well after the patient had been exceedingly ill.

Orbital Tumour: Case for Diagnosis.—W. E. HEATH, M.B., B.S. (shown by J. MAZELL, M.B., Ch.B.).

This man attended hospital a month ago, his only complaint being that during the last six months the left upper lid had fallen, and the eye had dropped in the orbit. He has not complained of pain or of inflammatory symptoms. In the ordinary routine a skiagram was taken, and the startling rarefaction [demonstrated] was seen laterally to the left frontal bone. The lateral skiagram showed that an egg-shaped area of lesser density was receding from the frontal bone, and seemed to be lying on the roof of the orbit, in the anterior fossa of the skull.

The patient's vision has not materially deteriorated. The eye movements are fairly good in all directions except upwards, where there is slight limitation. He was under observation in hospital for two weeks, and during the whole period his temperature was normal.

There may be a tumour not connected with the eye, but arising from the dura mater of the anterior fossa, and our tentative diagnosis is endothelioma or meningioma, which has pushed up the brain and rarefied the bone, without invading it. We feel that we can rule out any infective condition, as there is no definite limitation of ocular movement and no rise in temperature. There seems to be no connexion with frontal or other sinuses, and the result of the throat examination is negative.

He is now in St. Thomas's Hospital under Mr. R. H. Boggon, for further investigation, and it is proposed to explore the region of the anterior fossa.

Traumatic Retinitis Proliferans.—A. RUGG-GUNN, F.R.C.S.

E. H., female, aged 10 years.

History.—Fell against the corner of a chair at age of ten months. The left eye was injured and was for a time proptosed and divergent. The vision of that eye has been known to be defective for three years.

Present condition: Right vision = $\frac{6}{6}$, left vision $< \frac{5}{60}$.

Pupils react. There is considerable deviation upwards of the left eye, but movements are unimpaired.

On examining the left fundus a bluish white mass is seen to occupy the central portion of the vitreous. Near the disc to which it is attached, this mass is roughly cylindrical in shape. In the anterior vitreous it ends in a rosette of pointed processes, from which taut, delicate fibrils pass forwards to the back of the lens and probably to the anterior fundus. The straight appearance of these fibrils suggests tension, but examination with the slit-lamp shows a series of perfectly slack, delicate attachments to the back of the lens. The child, however, is not a good subject for observations with the slit-lamp and it is possible that some of the attachments may be tense.

There is evidence of degenerative choroiditis, without pigment, in the fundus, especially above the disc, and the appearance is suggestive of partial rupture of the optic nerve.

Points of interest about this case are: (1) The absence of visible vessels in the new formation; (2) the absence of retinal detachment. The history of injury is too definite to ignore and it suggests that the condition has been present for nine or ten years.

I have shown the case mainly to obtain information on the life-history of retinitis proliferans.

Mr. D. V. GIRI described a somewhat similar traumatic case in which perfect vision had resulted without the formation of permanent fibrous tissue.

Staphyloma of Cornea with Glaucoma.—A. C. HUDSON, F.R.C.S.

John D., aged 14 years.

History.—The patient was first seen when nine months old. There was extensive scarring of both corneæ from perforation following ophthalmia neonatorum, with anterior synechia of entire pupillary border, abolition of anterior chamber, and increased intra-ocular pressure. The right cornea was becoming staphylomatous. Corneoscleral trephining was performed once on the right eye and twice on the left, and iridotomy, so as to form an artificial pupil, on both eyes. The sight of the left eye continued sufficient for the child to ride a bicycle until four years ago, when it failed rapidly after tonsillectomy.

Vision in each eye is now little more than "hand-movements" close to the eye. In the right eye there is lens opacity, and projection of light is very defective in the nasal field, but this eye is now the better of the two, and the child does not remember ever having seen much better with it. Projection of light in the left eye is defective and no red reflex is obtainable. The tension of the right eye is considerably raised, that of the left normal.

Should an attempt be made to arrest the glaucoma in the right eye?

Discussion.—Mr. TUDOR THOMAS said that not long ago he had a case which was somewhat similar, but without increased tension. In that case there was a fairly dense opacity of the cornea, coloboma of the iris, anterior synechia, and lental opacities. Vision in the affected eye was a little better than in the present case; the patient could count fingers at eight to ten inches. After needling of the lens, a good fundus reflex was obtainable, and vision improved to finger-counting at four feet two inches. In Mr. Hudson's case, if the tension of the eye could be dealt with satisfactorily, it would be worth while needling the lens, after which some improvement might be expected, though he did not think the improvement was likely to be so great as in the case to which he had referred.

Mr. J. GRAY CLEGG recommended using a 2 mm. trephine and taking the whole disc out, as there was the possibility that the hole would fill up with scar tissue in such a case as this.

? Sarcoma ? Tuberculoma of Iris.—G. HANDELSMAN (introduced by Mr. ARNOLD SORSBY).

R. F., a boy, aged 9. Left eye became inflamed three weeks ago. When first seen ten days later the cornea was so hazy as to suggest a diagnosis of interstitial keratitis. A hyphæma could be seen. Three days later, under the effect of atropine, the mass on the iris could be distinguished.

Points of special interest are: Vascularization of mass and adjacent iris tissue; evidence of iritis; absence of keratitis punctata; normal fundus. The Wassermann reaction is negative.

Discussion.—Mr. J. GRAY CLEGG said that the swelling seemed to be translucent, and therefore he thought that it might possibly be a cyst of the iris, which had given way, and

a vessel had been torn across, causing hyphæma. A granuloma displacing all the pigment of the iris at its site would, however, also be translucent.

The PRESIDENT said the case reminded him of one which he had had many years ago, microscopical sections of which he had shown at a meeting of the Ophthalmological Society. That had turned out to be an inclusion cyst, with one or two microscopical fragments of wood. He removed the cyst. His first idea of the condition in the present case was that it was tuberculoma. The cyst in his own case had followed collision with the leg of a table.

Mr. R. C. DAVENPORT said he only saw the patient three days ago, and then the eye was more inflamed. He thought the swelling was unlikely to be sarcoma; his opinion was that it was an inflammatory process, whatever the cause of that might be.

Section of Urology

President—J. B. MACALPINE, F.R.C.S.

[*October* 25, 1934]

The Musculature of the Bladder-neck of the Male in Health and Disease

PRESIDENT'S ADDRESS

By J. B. MACALPINE, F.R.C.S.

ABSTRACT.—Two muscles, one voluntary the other involuntary, and an inch or more apart, exist at the bladder-neck, both functioning as sphincters. This dual musculature is explained by the emergence of the ejaculatory ducts between them. If both these muscles are open when semen is discharged, secretion may escape into the bladder or to the exterior; also any urine in the bladder would flow out. The internal sphincter is usually tightly contracted during ejaculation. Instances are given in which the internal sphincter is paralysed or injured, e.g. after operation, in prostatic hypertrophy, and in certain nervous diseases. Two instances are reported of direct injury to this localized area, with the result, in all cases, that ejaculation failed. In prostatic hypertrophy the internal sphincter is dilated by the growing adenoma, and at operation it is further stretched and perhaps cut across. Continence then depends on the external sphincter. Two cases are reported in which the external sphincter had previously been paralysed, total incontinence resulting.

Recent work on the trigonal muscle; its function in opening the internal sphincter. Influence of this muscle on prostatic enlargement.

In central nervous disease the dilatation of the internal sphincter resulting in funnel-neck is very common. Series of cases of nervous disease examined in order to determine the frequency of this sign are reported. Other cases occurring apart from nervous disease are also reported.

The part played by the sphincters in ejaculation is discussed and illustrated by reports of further cases of paralysis of the sphincter. It is shown that in the normal way the external sphincter weakens during coitus, and especially during ejaculation, but when the internal sphincter is paralysed no weakening is evident. Penile erection is associated with spasm of the internal sphincter, and spasm of the internal sphincter is produced by irritation, e.g. vesical stone may produce priapism.

The varying types of adenoma bear different relationship to the internal sphincter. The view that submucosal glands give rise to prostatic enlargement is supported. The influence of prostatic enlargement on the internal sphincter is discussed.

RÉSUMÉ.—Il existe deux muscles, l'un strié et l'autre lisse, séparés d'un pouce, au col de la vessie; tous deux fonctionnent comme sphincters. Cette double musculature s'explique par l'ouverture du canal éjaculatoire entre les deux muscles. Si les deux sphincters sont ouverts au moment de l'éjaculation du sémen, la sécrétion peut s'échapper dans la vessie ou à l'extérieur, et l'urine dans la vessie s'écoule. Le sphincter interne est d'habitude fortement

contracté pendant l'éjaculation. Il existe des cas où le sphincter interne est paralysé, par exemple après une opération, dans l'hypertrophie prostatique, et dans certaines maladies nerveuses. L'auteur décrit deux cas de traumatisme direct de cette région, tous deux suivis d'éjaculation manquée. Dans l'hypertrophie prostatique le sphincter interne est dilaté par l'adénome croissant, et il est encore plus détiré, et peut-être sectionné, à l'opération. La continence dépend alors du sphincter externe. Description de deux cas où le sphincter externe était déjà paralysé avant l'opération, résultant en incontinence absolue.

Travaux récents sur le muscle trigonal, sa fonction dans l'ouverture du sphincter interne. Influence de ce muscle sur l'hypertrophie prostatique,

Dans certaines maladies nerveuses la dilatation du sphincter interne, menant à la condition de col en entonnoir, est très fréquente. Rapport d'une série de cas examinés pour déterminer la fréquence de ce signe. D'autres cas survenant en dehors des maladies nerveuses sont aussi décrits.

Le rôle des sphincters dans l'éjaculation est discuté et illustré par d'autres cas de paralysie du sphincter. Il est démontré que normalement le sphincter externe s'affaiblit pendant le coït et surtout pendant l'éjaculation, mais si le sphincter interne est paralysé cet affaiblissement ne se produit pas. L'érection du pénis est accompagnée de spasme du sphincter interne, et le spasme du sphincter interne est produit par l'irritation. Par exemple, la calculose vésicle peut produire un priapisme.

Les différents types d'adénome ont des rapports différents avec le sphincter interne.

La théorie que les glandes sous-muqueuses produisent l'hypertrophie de la prostate est soutenue. L'effet de l'hypertrophie prostatique sur le sphincter interne est discuté.

ZUSAMMENFASSUNG.—Es gibt zwei Muskeln, am Blasenhals, ein willkürlicher und ein unwillkürlicher; beide sind Sphinktermuskeln. Diese doppelte Muskulatur is dadurch erklärt, dass der Ductus ejakulatorius zwischen diesen zwei Muskeln ausmündet. Wenn beide Muskeln offen sind während der Ejakulation kann das Sekret in die Blase oder auswärts entgehen, und der Harn in der Blase fliesst hinaus. Der innere Sphinkter ist gewöhnlich während der Ejakulation fest geschlossen. Verf. gibt Beispiele von Lähmung oder Schädigung des inneren Spinkters, wie nach einer Operation, bei Prostatahypertrophie, und bei gewissen Nervenkrankheiten. Zwei Fälle von direktem Trauma in dieser Gegend werden besprochen, mit Misslingen der Ejakulation in beiden Fällen. Bei Prostatahypertrophie wird der innere Sphinkter durch das wachsende Ademon gedehnt, und wird bei der Operation noch weiter gestreckt, und vielleicht durchgeschnitten. Die Kontinenz hängt dann von dem äusseren Sphinkter ab. Verf. beschreibt zwei Fälle vorausgegangener Lähmung des äusseren Sphinkters, bei denen totale Inkontinenz erfolgte.

Neuere Arbeiten über den Trigonalmuskel; seine Funktion in der Offnung des inneren Sphinkters. Einfluss dieses Muskels auf der Prostatahypertrophie.

Bei Erkrankungen des Zentralnervensystems ist der trichtesförmige Blasenhals infolge Erweiterung des inneren Sphinkters sehr häufig. Eine Reihe von Fällen wurde untersucht um die Häufigkeit keit dieses Zeichens festzustellen. Andere Fälle, ohne Nervenerkrankung vorkommend, werden auch beschrieben.

Die Rolle der Sphinkteren in der Ejakulation wird beschrieben und durch weitere Fälle illustriert. Verf. zeigt dass der äussere Sphinkter während dem Koitus normalerweise geschwächt ist, doch kommt diese Schwächung bei Lähmung des inneren Sphinkters nicht vor. Erektion ist mit Spasmus des inneren Sphinkters verbunden, und Spasmus des inneren Sphinkters ist durch Reisung bedingt. Es kann, zum Beispiel, Blasenstein Priapismus bedingen.

Die verschiedenen Typen von Ademom haben verschiedene Beziehungen zu dem inneren Sphinkter. Die Annahme dass submükose Drüsen Prostatahypertrophie bedingen ist bestätigt. Der Einfluss der Vergrösserung der Prostata auf dem inneren Sphinkter ist erörtert.

For some time past the surgery of the bladder-neck has enjoyed a fair share of the centre of the stage, but the subject of this paper is a phase of bladder-neck surgery which has enjoyed no such publicity.

In the first place I wish to draw attention to an arresting fact, namely that two muscles, separated from each other by an inch or more, exist at the bladder-neck, both functioning as sphincters, the one an unstriped, involuntary muscle, receiving its

nerve-supply from the sympathetic and parasympathetic, the other a striped muscle under the control of the will and innervated by the internal pudic, a spinal nerve. Do these two strangely different muscles play an identical part in the economy of the organism, and if so, why are they so strangely dissimilar? Do they merely support and understudy each other in their functions?

It is to a study of this problem that my paper is in the main devoted. The dual musculature of the bladder-neck is, of course, explained by the emergence of the ejaculatory ducts between the two sphincters. The posterior urethra is the point of meeting between the urinary and the genital streams, and it is to this confluence that we owe the complication of a dual musculature. It is obvious that if both these muscles were open at the time when semen was being discharged from the ejaculatory ducts, that secretion would be at liberty to escape, not only to the exterior, but also into the bladder, whilst any urine which happened to be in the bladder would flow out with the semen. If, on the other hand, both sphincters were closed, sperm would be imprisoned between them. It seems that in health, with the onset of penile erection, the internal sphincter, relatively weak muscle though it is, goes into spasm and in this way cuts off the urinary tract at the bladder-neck from the genital tract, thus imprisoning the bladder contents and forbidding their interference with the sexual act. The nervous centres for erection of the penis and for closure of the internal sphincter appear to be closely related, as will be attested more than once during the present paper.

I shall now proceed to the discussion of several conditions in which the bladder-neck musculature undergoes change, commencing with that source of never-failing interest, prostatic hypertrophy.

Prostatic Hypertrophy

The influence of prostatic enlargement on the musculature of the vesical neck is an important one, two muscles being principally involved—the internal sphincter and the trigonal muscle.

The internal sphincter.—The relationship of prostatic hypertrophy to the internal sphincter is a variable one. It is also an important one—so important that the anatomical type of the enlargement is determined thereby. The characteristic thing, of course, is for the internal sphincter to be displaced, so that it comes to lie half an inch or more down the dorsal aspect of the middle lobe. The growing adenoma appears to worm itself through the muscle ring so that a large proportion of the gland projects within the vesical cavity. The enlargement therefore consists of an intravesical and an extravesical portion. The encroachment of the prostate on the bladder cavity and its relationship to the vesical sphincter was difficult or impossible to explain so long as it was believed that prostatic enlargements sprang from the main body of the prostate gland. It is now a widely held opinion that the point of origin of most prostatic hypertrophies is in certain glands situated below the mucosa of the trigone and urethra (fig. 1, p. 4). Though they form a practically continuous chain it is found convenient to classify these glands into three groups according to their anatomical position. They are known, therefore, as subtrigonal, subcervical, and prostatic glands. The "subcervical" group, which is the largest and most important collection, lies at the level of the uvula vesicæ. It is also called "Albarran's" group because to this French surgeon falls the credit of having first demonstrated its rôle in the production of the so-called middle lobe of prostatic hypertrophy. Glands placed along the urethra are found all round that tube but are largest and most numerous on its floor. They are accommodated in the submucosal layer and are therefore internal to the muscular coat, though a few of them penetrate between the surrounding muscular fasciculi. If an enlargement starts in a structure occupying this situation there is no difficulty about explaining the relationship of the adenoma to the internal sphincter and how that muscle comes to

be displaced. It originates actually within the circle of the muscle and by its growth it expands the latter. According to which sub-group of glands is primarily or mainly affected will work out the relationship of the new growth to the internal sphincter

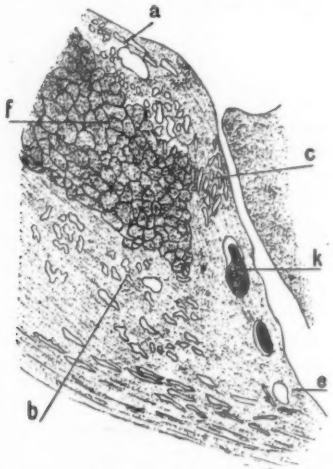

FIG. 1.—Sagittal section of the urethra and vesical neck in the earliest stage of prostatic hypertrophy (Jores), showing : (*a*) Submucous glands, subcervical group ; (*b*) submucous glands, intermediate group ; (*c*) submucous glands, urethral group ; (*e*) urethral mucosa ; (*f*) sphincter ; (*k*) glands in process of cystic transformation.

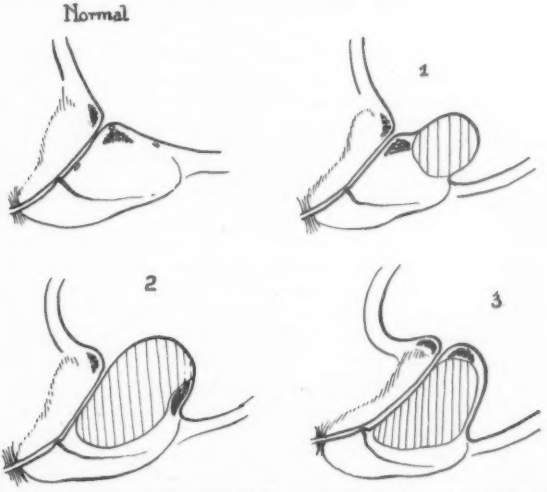

FIG. 2.—Illustrating the relationship of the various anatomical types to the sphincter. In the drawing of the normal, three small circles show diagrammatically the position of the subtrigonal, subcervical and urethral groups of submucous glands.

(fig. 2). The subtrigonal group lies above the sphincter and if it bears the chief brunt of the enlargement, the intravesical part will be disproportionately large ; if the subcervical group, which lies within the circle of the muscle, is principally affected, the

internal sphincter will be much dilated and the growth will be equally distributed within and without the sphincter, that is its intra- and extra-vesical components will be approximately equal. Should the prostatic group of glands, which lies some distance down the urethra and entirely below the sphincter, alone be affected, the hypertrophy never gets within the sphincter and that muscle remains undilated, the only displacement which it suffers being one of elevation, due to increasing size of the underlying prostate. Of these three types the second is by far the most common. The first and third are very rare in the pure form, though either type, or both together, are frequently found in association with Type 2. If Type 1 occurs alone—a great rarity—you enucleate a middle lobe without any lateral lobes and the whole hypertrophy occurs above the sphincter. If Type 3 occurs alone dilatation of the sphincter is absent and the enlargement is entirely subsphincteric. Returning to the more common type, which arises in the subcervical glands of Albarran, we find that in front of the meatus the sphincter is relatively little, sometimes not at all, disturbed; it often continues to abut on the meatus. As we trace the muscle from before backwards on the lateral aspect of the hypertrophied prostate we find that its displacement steadily increases, the lateral lobes of the gland gradually emerging through the sphincter, though in less measure than the middle lobe. In favourable

FIG. 3.—Anterior view of enucleated adenoma to show relationship of gland to internal sphincter. Note that sphincter is but little displaced anteriorly but recedes posteriorly. Also observe the groove which it makes on the adenoma.

circumstances when the bladder is fully displayed at operation the position of the sphincter may be traced as an oblique line marking off the mucosa behind, which properly belongs to the bladder, from that in front which overlies the encroaching gland and which is derived from the urethra.

At operation the enucleating finger passes between the adenoma and the sphincter. Often, however, a ribbon of muscle fibres tears away and adheres to the growth (fig. 3). These adhering fibres are scanty in number and constitute but a small part of the total muscle, though they are sufficient to determine its relationship to the gland. The site of the sphincter is always discernible, if a careful examination is made, as a shallow groove on the surface of the removed adenoma but in numerous instances there is a quite well-marked furrow which clearly shows that the pressure of the muscle is not a negligible force and that it evidently plays a material rôle in pressing together the various lobes of the abnormal gland and probably thus helps to obstruct

the outflow of urine. Is it a revolt of this misused muscle which determines that final acute retention which may set in so dramatically? And if so, would a simple division of this muscle band at one or more sites be enough to relieve the patient's symptoms? I have no answer to these questions. I have often thought of experimenting with this limited operation but have not had the courage to face the risk of failure.

The bladder mucosa proper extends forward only as far as the new position of the internal sphincter. In front of this is mucosa drawn up from the urethra overlying the invading prostate. But little submucosal tissue separates the adenoma and mucosa at this site and the mucosa is thin and fragile. It was on these facts that Freyer relied when he recommended the surgeon to scratch through the mucosa just behind the urethral orifice. But, as we have seen, a certain number of glands do not displace the sphincter and when this is the case the finger encounters firm muscle. Under these circumstances it becomes a bad method of approach. Great difficulty will be encountered in effecting an entrance and when an entrance is effected it will not lead the surgeon's finger to the outer surface of the adenoma where a welcome line of cleavage awaits it. It is many years since I forsook Freyer's method of approach in favour of the intra-urethral access which I find very much more expeditious, more certain and easier. I expect that most other surgeons have likewise adopted this route, but I sometimes wonder if ignorance of this elementary fact may not account for the considerable number of cases of non-adenomatous bladder-neck obstruction which seems to occur in the practice of some writers, a condition which is a great rarity in my own experience. I may be quite wrong but I hazard the view that with some operators the extravesical, that is the subsphincteric, type of adenoma swells the numbers of cases of "prostatisme sans prostate." I am not alone in suspecting that "prostatisme sans prostate" is a rare disease, for other surgeons—amongst whom I may quote Marion of Paris and Zuckerkandl of Vienna—have expressed similar opinions. These writers, whilst they do not entirely deny the possibility of the occurrence of such conditions, have each insisted on their rarity, and consider that the diagnosis should be made only after a most exhaustive attempt to exclude other conditions.

In the operation which I favour a horseshoe incision is traced by an electric needle and circles posteriorly behind the meatus. From the centre of this a second incision passes directly backwards for half an inch. The last incision is a deep one and divides many, perhaps most, of the fibres of the muscle. I believe there is no danger in going deeply at this point for if you examine a sagittal section of the bladder-neck from a patient with prostatic disease you will find a great depth of tissue in this situation. The object of this last incision is, of course, to prevent stricture formation at the bladder-neck and the principle was introduced by Sir John Thomson-Walker, with this intention in view, though he made his incision at a later stage in the operation. Its object is to lay into one, the cavity previously occupied by the adenoma and the bladder cavity, and in doing this the continuity and function of the internal sphincter are necessarily, in greater or less measure, destroyed. If you examine a prostatectomized patient some time after healing is complete, using a cystoscope or urethroscope, you will find the adenoma cavity shrunk to the size of a marble and communicating freely through a moderately wide aperture with the bladder. The internal sphincter is no longer competent and bladder continence depends entirely on the external. The importance of the external sphincter is illustrated by two cases in my practice in which continence failed.

Mr. —— was referred to me by Dr. Halsall of Colwyn Bay, suffering from hæmaturia and retention of urine. Ten years previously a rectal carcinoma the size of a shilling had been treated by the removal of the rectum and a colostomy functioned in the left iliac fossa. Prostatectomy was performed and convalescence was smooth in spite of the colostomy opening, but total incontinence was the unfortunate outcome of an otherwise

successful operation. It appears to me certain that, during the operation on the rectum, damage to the external sphincter or its nerve supply had resulted in its permanent damage or paralysis. Between the time of the rectal operation and the prostatectomy the patient had relied on the internal sphincter only, and that muscle was destroyed by the prostatectomy.

I wonder if other members have knowledge of a prostatectomy undertaken after a Kraske and what the result was. Bladder function is, as is well known, very liable to be interfered with for a time after the removal of the rectum and I remember on more than one occasion being pressed to remove a prostate to rectify this trouble. In each instance, however, micturition has been re-established after an interval of waiting and the case just reported is the only one within my experience in which the dual operation has been performed, the interval between the two operations in this case being ten years.

The only other case in which I have encountered incontinence after prostatectomy was that of a patient whose perineum and ischiorectal fossæ had, at an earlier date, been the site of severe tuberculous disease. The reason for this patient's lack of control is not so evident as it was in the former case but presumably is related to involvement, by the preceding tuberculous disease, of some part of the neuro-muscular mechanism of the external sphincter, on which we mainly rely for continence after suprapubic prostatectomy.

A period of years elapsed between the time when each of these men lost the use of his external sphincter and the time when they started to suffer from prostatic obstruction. During these years neither was conscious of any alteration in the act of micturition, which remained fully under the direction of the will. In other words two patients whose external sphincters were known to be paralysed had complete voluntary control by means of their internal sphincters—muscles composed of plain fibres, innervated by the sympathetic and parasympathetic—muscles, the voluntary control of which is often questioned. In view of this observation the discovery by McCrea of considerable amounts of striated muscle in the internal sphincter is significant. Now we have already seen that the prostatectomized patient, who has lost his internal sphincter, similarly has complete vesical control, so that it would appear to be a matter of indifference, so far as micturition is concerned, to have either of the sphincters out of action, so long as the other retains its normal capacity.

In a large number of prostatectomies these are the only two cases in which I have had the misfortune to render the patient incontinent, and in each of them an extraneous cause was responsible. Such an experience speaks volumes for the reliability of the external sphincter after the suprapubic operation and incidentally demonstrates how far it is removed from the operative danger zone. It is the external sphincter alone on which we can depend for post-operative continence and at all costs it should be safeguarded from injury. I have no personal experience of the perineal approach to the prostate and ought therefore to be hesitant in speaking upon this operation, but to my mind it carries the fundamental objection that it endangers the compressor urethræ or its nerve supply. The internal sphincter has suffered pre-operative change and dilatation in the way which I have described. It may be true that further damage is not inflicted upon it by surgical division, but even so there is no guarantee that it will recover its tone and will once more function as a sphincter. An American speaking in London last year remarked that he "understood the mortality rate for perineal prostatectomy to be lower than that for the suprapubic operation, but many of those who lived wished devoutly that they hadn't."

How does prostatectomy affect sexual potency? Leaving out of consideration the integrity of the ducts and the sufficiency of prostatic tissue—both of them questions which are surely finally settled—I shall confine my remarks to the function of ejaculation. It is obvious that the injury to the internal sphincter will destroy

bladder closure in many cases and will allow sperm to regurgitate into that viscus instead of diverting it to the exterior. With most prostatic patients sexual life is a thing of the past, but to a few, especially the younger ones, it is still interesting.

A man, aged 50, who was referred to me with complete prostatic retention, had recently married for the second time. He was concerned to know the effect of operation on his sexual capacity. I attempted to explain the position to him and asked him to keep in touch with me after his discharge from the home and to report on his experiences. During the operation I was at some pains to avoid injury to his internal sphincter. Several months after he left the home he reported to me that in the first period coitus was accompanied by an orgasm but by no emission. At a slightly later time, however, the act became completely normal and was accompanied by normal ejaculation.

It can scarcely be doubted that these two phases in this patient's recovery correspond to stages in the recovery of his internal sphincter. When circumstances have been suitable, I have taken the opportunity to seek information on this subject and have made a few similar observations, but taking into account the large number of prostatic patients upon whom one operates, it is astonishing how infrequently the chance to study this question presents itself. The reasons are to be found, I suppose, in the age of the patients and in their indifference or perhaps reticence. Of those from whom I have obtained a precise answer the majority appear capable of coitus and experience an orgasm, but they remain incapable of ejaculation, though some of them realize that semen has been shed internally. Only a minority exhibit a return to full sexual capacity.

It would appear from this that, following prostatectomy, two conditions of the internal sphincter may occur: the one in which the sphincter is intact, when it remains capable of performing its sexual function and probably also its function as a sphincter of the bladder; the other in which the muscle has been permanently injured. Each of these conditions can be verified time and again at cystoscopy or urethroscopy. The second state must certainly be more common with the modern operation, in which the sphincter is intentionally sacrificed, than it was with the more limited technique previously employed. Yet, more than twenty years ago, presumably therefore in cases in which the old technique had been used, Thomson-Walker, approaching the question from the view-point of the capacity of the muscle to retain urine in the bladder, showed that the internal sphincter became active and competent in considerably less than 50% of cases.

The trigonal muscle.—This is a muscle about which fresh ideas have been advanced recently, and if they prove acceptable our views regarding the physiology of micturition must be modified. It is interesting to note that in his report at the International Congress of Urology last year Kenneth Walker was able to provide certain new facts in support of this theory.

Till recently it has been taught that at the onset of urination the internal sphincter undergoes an "inhibitory dilatation," comparable to that seen at the anal sphincter, but according to Young and Wesson the trigonal muscle, superficially placed in reference to the other muscles of the bladder-neck, passes downwards and forwards through the internal urethral orifice to receive an insertion near the verumontanum (fig. 4). Its course between these two points is not in one plane, the vesical and urethral sections being set at an angle, which angle is situated at the internal meatus of the bladder and corresponds to the uvula vesicæ. In contracting, the trigonal muscle straightens out this angle and drags back the lower lip of the orifice of the bladder. The internal sphincter is dragged open and micturition commences.

McCrea has shown that the trigonal muscle is a thin sheet whilst the internal sphincter is a very definite structure. If, therefore, the trigonal muscle has the action which Young and Wesson attribute to it, a relaxation of the internal sphincter

must accompany the contraction of the trigone. There are many elastic fibres helping to close the bladder-neck in the neighbourhood of the internal sphincter. It is possible that the sphincter muscle itself relaxes but that the trigonal muscle is responsible for the holding open of these elastic fibres.

Young and Wesson describe the downward action of the trigonal muscle in straightening out the angle formed at the uvula vesicæ but scarcely mention the

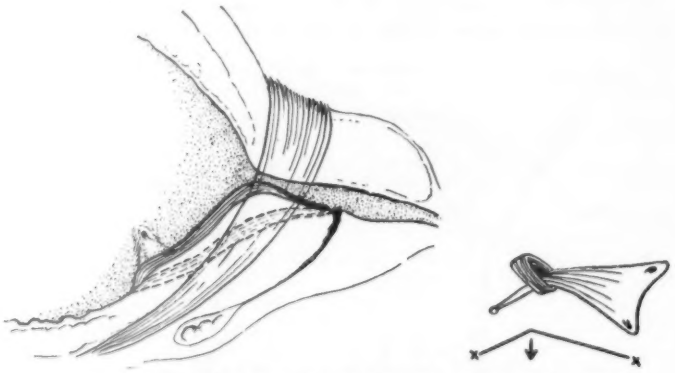

FIG. 4.—Sagittal section of bladder to show trigonal muscle at rest and (dotted lines) in action. (Modified from Young.)

importance of its triangular shape. The two bars of Bell converge on the urethral meatus and as they enter this opening alter their course and run side by side down the urethra. When contracting these two divergent limbs must become straightened out and thus exert an outward as well as a downward traction on the internal meatus, in this way opening the meatus transversely as well as vertically (fig. 5).

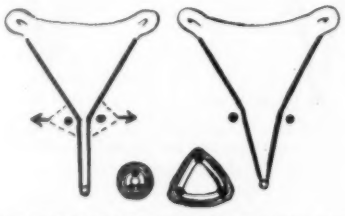

FIG. 5.—In contracting the divergent limbs of the trigone exert an outward traction on the meatus.

In prostatic enlargement the trigonal muscle hypertrophies to overcome obstruction. But what is its relationship to the growing adenoma? The insertion of this muscle we have seen to be in the neighbourhood of the verumontanum. Is its urethral component displaced back as is the internal sphincter, or does it run through, or over, the neoplasm? The two muscles of Bell normally converge on the uvula vesicæ, and it is underneath this eminence that Albarran's glands are situated. When hypertrophy affects these glands they push up between, beneath, and around

the Y-shaped convergence of the two muscles of Bell (fig. 6). If you cut sections in this area you will find that the cleft between the middle and lateral lobes is occupied by a layer of longitudinally running muscle, the like of which you will not find over the rest of the gland surface, and this bundle is continuous all the way down the cleft to where it meets its fellow. This muscle is the direct continuation of the bundle which lies in the bar of Bell, and is, I believe, the self-same muscle which, in the normal condition opens the internal sphincter, as described by Young and Wesson. These muscles, therefore, lie in the vallecula between the middle and lateral lobes, and almost certainly the resistance they put up to the growth of the

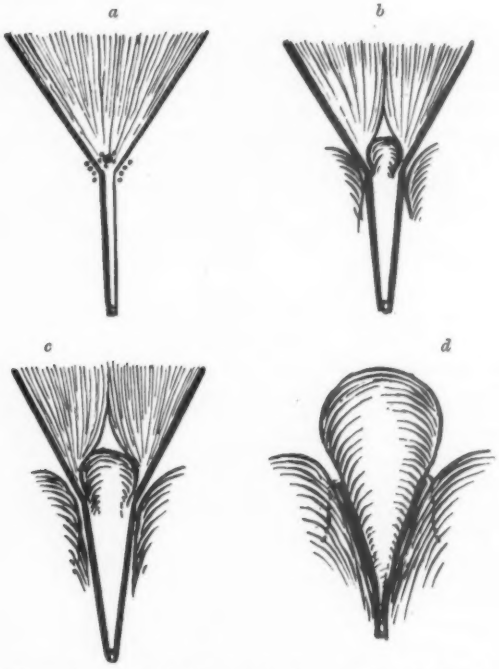

FIG. 6.—Shows the glands of Albarran in the normal and in varying grades of hypertrophy. In fig. *d* a retroprostatic pouch has formed and the trigonal muscle doubles back acutely into it. (Cf. fig. 7.)

adenoma is the cause of the furrows which they occupy, and therefore the cause also of the prostatic division into the lateral and middle lobes. The subdivision of the prostate into lobes has never been satisfactorily explained but the presence of muscle bundles within the clefts seems sufficient to account for it, and is, so far as I am aware, a new observation.

In health the internal sphincter and the trigonal muscle cross each other at the internal meatus, and here they are placed in close contact. In prostatic hypertrophy the internal sphincter is dislocated downwards and backwards by the adenoma, but the relationship of the two muscles is still maintained, for the sphincter lies at the point where the interlobular cleft emerges into the bladder cavity and the contained

muscle of Bell is bent over it in crossing (fig. 7.) The deeper the retroprostatic recess, the more acute the angle formed. Perhaps the hypertrophied trigonal muscle even plays a part in the dislocation of the internal sphincter. These muscular strands are torn across during the enucleation. The torn ends, in healing, probably acquire further attachments in the neighbourhood of the internal sphincter, but as that muscle is usually incompetent after prostatectomy, they are no longer required to play a part in micturition.

Nervous Disease.

Disease of the central nervous system frequently causes derangement of bladder function, and it has been common knowledge ever since the days of Romberg that dysuria may be the first symptom to show itself as a result of various spinal conditions, and particularly in tabes dorsalis. When it occurs thus as an isolated symptom it is elusive and is likely to be diagnosed only after exclusion of other conditions. It is true that trabeculation of the bladder is a recognized sign of nervous disorder and that it has certain characteristics which distinguish it from other types of trabeculation, yet this trabeculation is inconstant and easily overlooked and its diagnostic significance is not completely reliable.

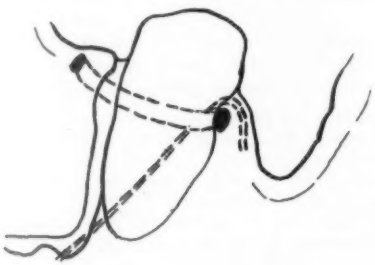

Fig. 7.—The urethral component of the trigonal muscle passes up between middle and lateral lobes and doubles back into the trigone. Note relationship to the internal sphincter, which, perhaps it helps to displace.

An early sign, of at least equal importance, is to be found at the bladder-neck. It results from weakness or paralysis of the internal sphincter, producing a funnel-shaped posterior urethra which can be seen cystoscopically. This phenomenon was apparently unknown clinically until 1917, when Burns of Baltimore described twenty-one cases of spinal syphilis (mostly tabes) in which he had observed the change. Prior to that, Barrington had demonstrated similar phenomena resulting from transsection of the cord in the cat. From the time that Burns described this cystoscopic appearance, onwards, the English and American press is almost completely silent, a single reference by my own colleague, D'Arcy McCrea, being all that I can find on the subject. On the European continent, six examples of funnel-neck bladder were reported in 1920 by Schramm, and the Germans, conveniently ignoring the priority of Burns' article, have christened the condition "Schramm's phenomenon."

In the healthy male it is impossible to see the walls of the posterior urethra when the cystoscope is withdrawn down the tube, because in so narrow a channel the prism and lamp are both in contact with the mucosa. In certain circumstances, however, the upper and lower walls of the channel are more widely separated, so that if the ocular end of the cystoscope is depressed and the window is turned downwards, the lower wall of the tube, together with the colliculus, can be clearly

seen (fig. 8). The anatomical basis of this condition is a widening of the proximal urethra, due to relaxation of the internal sphincter, the whole posterior urethra coming to form a funnel-shaped passage having its base directed towards the bladder and its

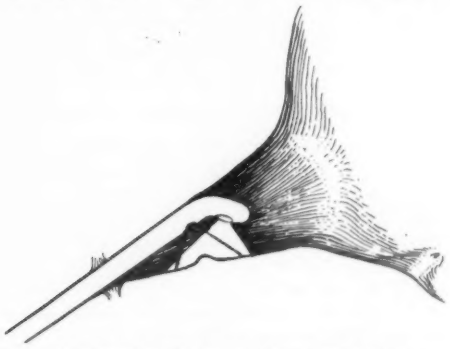

FIG. 8.—Cystoscopic examination of colliculus in "funnel-neck" bladder—type No. 4.

apex near the triangular ligament. This condition is variable in development, according to the degree to which the internal sphincter gapes, and Moro has described four grades (fig. 9) which are useful to keep in mind, as they form a good working basis:—

(1) In the first of these there is radial streaking in the neighbourhood of the bladder-neck, and the transition between the bladder and the urethra is more rounded and gradual than it is in the normal state.

(2) The canal shows early widening, and its lower wall, together with the colliculus, can be dimly discerned in the imperfectly lit channel.

(3) The field is at first dark, but as the cystoscope is further withdrawn it enters a relatively spacious chamber, where the lighting is good. On the floor of this chamber the colliculus is clearly visible.

(4) The condition is fully developed. The bladder orifice gapes widely, and the normal angulation at the transition of the bladder into the urethra fails. The posterior urethra can be visualized in its whole length.

Recently, Mr. Poole Wilson and I have examined a series of twenty-one patients suffering from nervous lesions, for the most part tabetics or general paralytics. It was originally my intention to discuss our findings at some length, but I unfortunately shall not have the time to do so. We propose to publish them in extenso shortly.

Disease	Number of patients	Principal symptoms		Grading (Moro)
Tabes dorsalis	15	Dribbling, Enuresis, Automatic micturition	7	4; 4; 2 → 4; 3; 3; 3 or 4 → 4; 1 or 2
		Difficulty	3	4; 0; 1 or 2
		No symptoms	2	4; 0 → 4
		Copious evacuation of urine	3	2 → 4; 4; 2 → 4
G.P.I.	4	Incontinence	2	3 → 4;
		No symptom	2	3; 4
? Tabes; ? G.P.I.	1	Sensation of distension without residual urine		0
Disseminated sclerosis	3			0; 0; 3
Spastic paraplegia	1			0

Suffice it to say that amongst twenty-one tabetics and general paralytics the phenomenon was positive in all except two, whilst an odd example turned up in a

case of disseminated sclerosis. Following Moro's gradings, we found ourselves very sceptical about Types 1 and 2 and only accepted the findings when Types 3 or 4 appeared. Repeatedly we observed that a minor degree of funnel-neck would develop into a major if the bladder was more fully distended, and this is indicated in column 3 of the table, where, not infrequently, two figures may be seen attached to one case. To obtain a good view of the colliculus is the best test of the sign and to do so the ocular end of the cystoscope must be sharply depressed. Otherwise the condition will be overlooked.

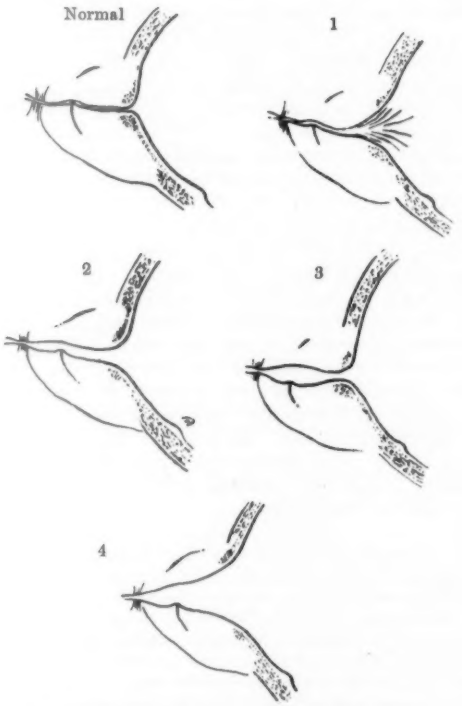

FIG. 9.—Illustrates Moro's gradings. (*See* text.)

In addition to these twenty-one patients who were known sufferers from central nervous disease, a watch has been kept upon our ordinary cystoscopic material. One hundred consecutive males examined during the present summer produced twelve examples of a funnel-neck. It is relatively easy to account for the phenomenon when discovered in a known paralytic, but when encountered unexpectedly it presents greater difficulties in interpretation and cannot then be explained in every case similarly.

Some of the cases are accounted for by straining to urinate—but then the condition is transitory—some, by the fatigue of prolonged cystoscopy. A few are due to spinal or sacral anæsthesia, but most of these have been checked by subsequent cystoscopy without anæsthesia. One or two may be cases of incipient nervous disease. Some of them are unexplained, so that the question must arise whether the sign is sometimes to be found in the completely healthy man.

This work shows how the internal sphincter bears the brunt of the paralysis in

nervous dysuria and appears to indicate that the external sphincter retains its function comparatively well.

In passing, it is worthy of remark that the internal sphincter, an involuntary muscle, is probably mainly responsible, in health, for continence during sleep, the external sphincter relaxing like other voluntary muscles. The paralysis of the internal sphincter, therefore, may play a part in the nocturnal incontinence which is so characteristic of tabes, though it must be admitted that this is not the whole truth, for the two patients whose war wounds I shall shortly describe are not incontinent at night, nor are those patients incontinent who have had their prostates removed and whose internal sphincter is out of action. We must postulate, therefore, the capacity of the external sphincter to take over this function in the absence of the internal sphincter, under favourable circumstances. That the circumstances are not favourable in tabes is obvious.

With most male tabetics, as is well-known, the capacity for sexual intercourse deteriorates. It remains intact, however, with a proportion. Fessler and Fuchs investigated fourteen cases of tabes who were still potent, and they obtained a clear history from three of them that intercourse and orgasm were normal but that there was complete absence of ejaculation, the flow of semen undoubtedly taking place into the patient's own bladder as a consequence of the breakdown of nature's barrier, the internal sphincter. These writers also state that where there was a severe lesion of the internal sphincter it became impossible to express the prostatic secretions by means of massage, in such a way that they would present at the external meatus, for the whole of the fluid regurgitated into the bladder.

We did not ourselves question all our patients about their sexual functions. Sometimes we were working in public-assistance hospitals with the nursing staff present, whilst not a few of the patients were of low intelligence. Two of the tabetics, however, were quite definite that orgasm without emission occurred routinely in their recent experience. On the the other hand, two patients with funnel-necks of No. 4 grade quite unexpectedly reported normal ejaculation.

I propose now to tell you of two unusual and interesting patients who suffered gun-shot wounds of their bladder-necks.

(1) Lieut. ———— was referred to me by Dr. McBain (Crumpsall). In September 1916, he was wounded in Delville Wood on the Somme. When walking near the front-line trenches about 5.30 in the evening he was hit by a rifle bullet which entered just behind the trochanter of the right femur. Three months later the bullet was removed from a site behind the left femoral trochanter, which fact probably serves to indicate accurately the line taken by the missile. He states that his bladder was full at the time and he experienced "extreme pain in trying to get rid of the water." Efforts at micturition proved unavailing, but some fæces were passed and these were noted to be blood-stained. His legs appeared to be paralysed, but he dragged himself back by his arms after dusk (8 o'clock). Catheterization was undertaken at an advanced dressing station and was repeated later. On the third day a suprapubic cystostomy was performed at a casualty clearing station (? 38th). He says that "there were no broken bones and the paralysis of the legs passed off after a few hours." There was no more blood from the rectum. The bladder healing was slow, and early in 1918, when still in hospital at Stratford-on-Avon, he developed a "septic kidney on the right side." After that he had intermittent febrile trouble and pyuria, and in 1928, his symptoms becoming more pronounced, I removed a pyonephrosis of the right kidney. After that the pyuria disappeared and the urine became quite normal. The febrile disturbances also ceased, and (seen within the last few weeks) he reports that he is in excellent health.

Prior to his operation (1928) he had discussed with me the fact that since he was wounded he had never experienced an ejaculation of semen, even though periodically sexual excitement led to a normal orgasm. As he was engaged to be married this circumstance gained additional importance. In view of this the endoscopy of his bladder and urethra, which I performed on three occasions, is of interest. I shall omit that part of the examination which concerns his pyonephrosis and urinary sepsis, as this is merely incidental,

and shall describe only the condition of the bladder neck. I think you will agree with me that the bladder shows wonderful powers of recovery from injury and that frequently it is difficult even to recognize the site where one has oneself operated. In this case I find it difficult to describe my findings. There was a loss of tissue affecting the posterior half of the meatus, such that the bladder cavity and the proximal portion of the prostatic urethra were laid into one, the trigone appearing to run flatly into the posterior urethra. An attempt to bring the colliculus into view was not successful. A posterior urethroscope of the Joly type confirmed this observation, the patent opening into the bladder being conspicuous and the prominence of the uvula vesicæ being absent. The urethral orifice appeared rigid and was roughly triangular in shape, the base of the triangle being placed postero-inferiorly. There is therefore little doubt that the bullet had crossed the neck of the bladder somewhere behind its mid-point and had destroyed the internal sphincter.

This patient has now been married for six years and states that coitus and orgasm appear to be completely normal, but that he has never had an emission. Asked whether coitus could occur when the bladder was full he replied in the affirmative. There has been no nocturnal incontinence.

(2) . . . . aged 34 was referred to me on August 16, 1931, by Dr. W. Fletcher Shaw, Professor of Gynæcology at Manchester University, who had been consulted by the patient's wife because she had been married for nine years and the marriage was childless. Inquiry showed the male partner to be at fault and the latter was advised to consult me.

In July 1917 this patient had been wounded at Ypres. During an attack he was emerging from a shell hole when he was struck by a rifle bullet. Apparently his thigh was in full flexion and the bullet entered above the knee on the inner and anterior aspect, traversed the subcutaneous structures for several inches, when it emerged, to re-enter over the right pubic bone and again emerge immediately to the right side of the coccyx. The right testicle and scrotum were destroyed in transit. After lying out for seven hours he lost consciousness. He states that his legs were not paralysed but he had no desire to get home as he was convinced that he was dying. He knows that his bladder was full before the attack began and after being wounded he experienced the greatest desire to urinate. He put his hand to the position of the coccygeal wound of exit and realized that a mixture of blood, fæces, and urine was exuding freely.

A catheter was passed at the casualty clearing station and a little blood-stained urine was withdrawn. Subsequently a suprapubic cystostomy was performed. From that time until the beginning of December, urine and fæces both came from the suprapubic wound. The bowel was then closed by operation. Shortly after this a piece of bone was removed from the bladder, after which progress was rapid. He was able to walk on Christmas Day. About midsummer he passed urine naturally. From this point convalescence was satisfactory until 1919, when he had what appears to have been a sharp attack of cystitis.

He married in 1922. Coitus proved to be normal in every way except that ejaculation was absent. His condition remained unchanged during the subsequent nine years, when he visited me. The patient says that spermatic fluid is unmistakable in the urine when it is next passed. He also states definitely that coitus can be undertaken when the bladder contains urine though it is never indulged in when the bladder is distended.

On examination.—He was a well-developed man. The entrance wound of the thigh was small and lay over Hunter's canal. The wound of emergence higher up the limb was several inches long and one to one and a half inches wide. It was covered with thin atrophic skin. There was considerable superficial scarring to the right side of the penis. The right testicle and the corresponding side of the scrotum were missing and there was a depressed area of scar, 2½ in. × 2 in., over the position of the right os pubis, which bone had been carried away. The thickened end of the ascending ischial ramus was easily felt in the groin. The wound of exit lay immediately to the right side of the coccyx and that bone was present, though apparently thickened owing to injury. There was a single rather wide suprapubic scar.

The urine was passed into two glasses and proved to be free from pus, albumen, or other abnormal constituent. There was no difficulty in micturition nor in commencing or controlling the act. He never wet the bed at night.

The cystoscope passed into the bladder without obstruction. The mucosa was healthy and the bladder normal in every particular with the exception of the region of the neck. On the right-hand side of the bladder outlet there was a deformity which evidently resulted from a loss of tissue. A notch was cut in the circle of the internal sphincter and the corresponding

portion of the right lobe of the prostate extending from 8 to 11 o'clock. This was slightly irregular in shape but was lined with healthy mucosa. The bite continued externally to involve an area of bladder wall which was sunken like a shallow, wide-mouthed diverticulum. On withdrawing the cystoscope into the prostatic urethra, that channel could be inspected only at the position where there was loss of tissue and the veru was not seen.

Attempts at artificial insemination were made by Professor Fletcher Shaw, the semen being recovered following coitus from the patient's bladder by means of an Ultzmann syringe, but these attempts proved unavailing.

This is a remarkable pair of cases, for it must be most unusual to meet instances in which this small but important area has suffered from a gunshot wound. The relative youth of these patients makes them more suitable material for the study of sexual functions than is the elderly prostatic or broken-down tabetic. The interest centres on the effect which the destruction of the internal sphincter has on emission. I would repeat that in each case coitus was normal except that emission failed and in each case intercourse could be safely undertaken, even when it was known that the bladder contained urine.

For purposes of contrast, I wish to cite another pair of cases reported by Brenner.

In these there was a gunshot wound of the spine resulting in an "automatic bladder," much residual urine and a "funnel-neck." Continence here again rested entirely on the external sphincter and though the voluntary control of that muscle had been lost the patients were completely continent both by day and night. Micturition was only possible by the manual expression of the bladder contents. The significant fact in these two cases was that in each the onset of an erection was accompanied by a slight trickling of urine, which continued till the erection subsided or until the normal level of residual urine was reached. Should ejaculation occur, the outflow of urine was more rapid immediately before and during the ejaculation. This phenomenon of micturition during erection, and especially during emission, is explained by Brenner on the assumption that simultaneously with the closure of the internal sphincter which accompanies erection, the external sphincter, which has no urinary function once the internal sphincter is in spasm, relaxes in preparation for emission. In the presence of a paralysed internal sphincter, bladder control was completely lost.

The examples of operative injury and of gunshot wound of the internal sphincter which I have been discussing previously, seem to indicate the behaviour after a simple localized injury of the internal sphincter. Brenner's cases, on the other hand, introduce a partial implication of the external sphincter. Reviewing these groups together, I think that it is fair to conclude:—

(1) That Brenner's cases show that with the onset of erection the external sphincter weakens and that the dilatation becomes more pronounced during emission, events which are not cause for surprise.

(2) On the other hand it is evident, from a study of the behaviour of the bladder sphincters in my own groups of cases, in which the internal sphincter alone was damaged, that the external sphincter does not relax sufficiently to allow sperm to pass unless the internal sphincter is tightly contracted, and it seems justifiable therefore to assume that a certain pressure must be developed in the posterior urethra before the signal is given for the external sphincter to yield and permit emission.

It is interesting in passing to observe that in coitus the relative efficiency of the two sphincters is the exact opposite of that occurring normally. Suppuration in the posterior urethra, when it has overdistended that department of the tube, finds an outlet, not through the external sphincter to the exterior, but through the internal sphincter to the bladder. This is a well-known clinical fact on which two or three glass tests depend in the diagnosis of posterior urethritis. The external sphincter is thus normally the more powerful muscle. But in coitus the position is reversed, the internal sphincter being tightly contracted and the external yielding so soon as

pressure rises in the posterior urethra. If, from any cause, the internal sphincter fails to play its part, regurgitation into the bladder takes place. Moreover it appears that the external sphincter does not in these circumstances open up at all, for, as I have shown, coitus can be effected even in the presence of urine in the bladder, which indicates clearly that the factors causing yielding of the external sphincter were missing.

In further illustration of the association between erection and closure of the internal sphincter may be advanced the observation that certain patients with bladder stones suffer from priapism. Swift Joly says that he has met with this symptom chiefly in youths about the age of puberty and that he has never seen it in adults. I have, however, known priapism prove troublesome in adults, a relevant example being that of a man aged 45, who presented himself with a phosphatic bladder-stone, three-quarters the size of a golf ball. There was a moderate degree of accompanying cystitis and definite frequency of micturition. This patient complained that he had for some months suffered from priapism which was prolonged and might be painful. When it was protracted the call to empty his inflamed bladder became imperative, but he then found micturition to be impossible until the erection subsided. Here again, therefore, we seem to meet the association between spasm of the internal sphincter, provoked presumably by the irritation of the stone, and erection of the penis, an alliance which appears to have its physiological basis in the necessity for bladder closure during coitus.

(3) When the sexual act is effective but no ejaculation eventuates, the patients invariably express the view that the orgasm is completely normal and satisfactory. This fact would seem to suggest that the orgasm is not dependent on the distension of the posterior urethra or the propulsion of spermatic fluids along the urethra. By exclusion, therefore, we are led to the view that it results from stimulation of that sensitive structure, the verumontanum, by the emerging semen.

By some authorities it is stated that in coitus the passage of sperm into the bladder is prevented by the turgescence of the verumontanum. I would submit that the various cases I have cited this evening prove that this is an insufficient barrier, if indeed it is a factor at all. The verumontanum remains unaffected by prostatectomy, it is not involved by tabes, nor was it injured in the two war wounds which I have recorded. Yet in all these conditions ejaculation fails. I doubt whether swelling of the verumontanum is even an accessory factor, for if such had been one of its functions it would have been placed at the upper end of the prostatic urethra and not in the middle or lower reaches. It is, in my opinion, undoubtedly to the internal sphincter that this important office is entrusted. Possibly if per-urethral surgery maintains its present meteoric advance, eugenic sterilization will soon be available by the cystoscopic division of the internal sphincter!

Much experimental work has been done in the past on the bladder and its sphincters and in this work British physiologists and urologists hold a proud position. I have carefully excluded this field from my address to-night. Ultimately the two provinces are one, and must eventually be found in complete harmony. Physiologists themselves would probably be the first to concede that the results of experimental work may gain much by a review of the clinical field, and even that they require checking by clinical observations, with which they are not always in complete accord. Thus, examples may be instanced of nerves carrying fibres to muscles which physiologically antagonize each other, or, at least, do not act simultaneously. The common experiment of stimulating such a nerve may throw into action muscles, or groups of muscles, which are incompatible or antipathetic. An instance of such apparent discordant result may be seen in the nervi erigentes, stimulation of which nerves simultaneously causes an erection of the penis and a contraction of the vesical detrusor, two processes which we have, during the present paper, repeatedly recognized to be physiologically opposed. We

as clinicians assume that such a nerve supplies groups of muscles which act separately or perhaps antagonistically.

To spend an hour therefore examining the problem solely from the functional and clinical standpoint has, I trust, not proved profitless.

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Section of Tropical Diseases and Parasitology

President—P. H. MANSON-BAHR, D.S.O., M.D.

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Whither Tropical Medicine? An Epitome of Scientific Activities in British Tropical Possessions, together with a Consideration of the Position which Tropical Medicine occupies in Scientific Medicine at the Present Time.

PRESIDENT'S ADDRESS

By P. MANSON-BAHR, D.S.O., M.D., F.R.C.P.

THE main discoveries in Tropical Medicine have been made; the laurel wreaths have been won by the pioneers, but, as years roll on, the prizes are harder to attain and the scientific path becomes more toilsome still. The unfolding of new truths by the aid of a few simple reagents and stains has become a thing of the past, and now scientific equipment, adequate for any results, has to consist of ponderously equipped laboratories with a travelling menagerie attached.

In a review of the scientific activities in British Colonies and Dependencies, Africa has the first claim to attention, for in this vast continent the essential tropical diseases are bred and spread and the problems which confront our workers are essentially similar.

Sierra Leone.—Sierra Leone maintains at Freetown the important outstation of the Liverpool School of Tropical Medicine in the Sir Alfred Jones Laboratory. From this Institution much research work of prime importance has emerged. I need hardly mention the question of localized metazoan immunity as shown by the researches on the Tumbu fly (*Cordylobia*), and it is to Professor D. B. Blacklock that we are indebted for the very complete survey (1927-1928) of the incidence of disease in this Colony. The enormous amount of labour which it entailed during the year and a half which it took to perform may be imagined. The chief outcome of this demonstrated the comparative absence of tuberculosis, the extreme prevalence of gonorrhœa, the rarity of recognizable classical syphilis and the existence of all three species of enteric infection amongst the native tribes.

The Gold Coast.—The ancient Colony of the Gold Coast, with a native population of over 3,000,000 and 3,500 Europeans, possesses a fine Medical Research Laboratory at Accra which is associated with pioneer work in yellow fever and in consequence of this work the tragic deaths of Adrian Stokes, Noguchi and Young. It was here that Stokes, Bauer and Hudson (1927-1928) clearly proved that the agent of yellow fever is ultramicroscopic and filtrable and can reproduce the disease in monkeys of the genus *Macacus*.

Hookworm infection is very common, but in this virile population it seems to cause little serious deterioration of health and we may note the prevalence of both forms of bilharziasis, while guinea-worm infection is a serious problem in territories where the water-holes are scarce and are used indiscriminately as bathing establishments, lavatories and drinking fountains, so that it is obvious that much more work in the prevention of this infection is required. The trypanosomiasis problem also exists and human and cattle infections are spread along the main trade routes and the former carried by *Glossina tachinoides*, as well as by *G. palpalis*.

Nigeria.—The medical welfare of Nigeria is of prime importance in this largest and most prosperous West African Colony, which, with the Mandated territory of the

Cameroons, shelters a population of over 19,000,000 and 8,200 Europeans. Registration, which is compulsory in Lagos and Ebute Metta, reveals a birth-rate of 28·6 per thousand and a death-rate of 16·5. There are twelve European hospitals in this large territory, with clinical laboratories at Lagos, Port Harcourt and Kaduna and at the African Hospital at Calabar.

The medical research centre is at Yaba, five miles outside Lagos. The utility of this fine venture has been somewhat impaired by the separation of this institute from the necessary intimate connexion with a hospital. In spite of this drawback, a considerable amount of original investigation has been undertaken in this laboratory. Here the life history of *Loa loa* in *Chrysops* was worked out by the Connalls and an enormous amount of original research work—on mosquitoes, trypanosomiasis, babesiasis in cattle, and spirochætosis in fowls—was carried out. Yaba was selected by the Rockefeller Yellow Fever Commission, which, under Dr. Beeukes, Dr. Bauer, and their associates has done so much remarkable work and has succeeded in proving that the virus is widespread beyond the limits of clinically recognizable disease. The protection test was elaborated by Beeukes, Bauer, and Mahaffy in 1930 and a yellow-fever-like disease with jaundice was recognized by the first-named worker together with Walcott and Kumm in the following year. The training of native medical assistants is undertaken here. The premises consist of two school buildings and a hostel accommodating twenty-four students. There is also a School of Pharmacy and the progress of this venture will be watched with much sympathy and interest. Nigeria too is vitally interested in trypanosomiasis and maintains a Tsetse-fly Institute at Gadau near Sherifuri.

The trypanosome of West Africa is believed to be invariably *Trypanosoma gambiense*, but a strain (known as Sherifuri K) isolated from the district has been found to resemble *T. rhodesiense*. Native dispensary attendants are trained at Gadau and some, indeed, show great aptitude in recognizing trypanosomes under the microscope, in making gland punctures, and in performing injections.

More work is necessary on the prevalence of enteric fevers and it is thought that they are more common than was previously suspected in Central Africa. Attention must also be drawn to the possible existence of sprue where it has not been previously suspected, for three cases were discovered in non-Europeans in 1931. The leprosy problem is serious, over 6,000 cases came for treatment in 1930 under the supervision of the Nigerian Secretary of the British Empire Relief Association. The natives themselves appear to recognize leprosy early in its course and, whilst appreciating its infectious nature, they yet live a communal existence in small ill-ventilated huts, often sleeping on the same couch and even wearing the same clothes!

Tropical ulcer, as in other parts of tropical Africa, is a terrible scourge. In 1930 no less than 42,546 cases were noted as receiving treatment and obviously much more intensive work will have to be devoted to this subject.

South Africa.—Though not strictly within the tropical belt, the Medical Institutes of the Cape Province and the Transvaal form a buffer between tropical and non-tropical medicine. The Rand Mines employ native labour from many parts of Central Africa so that the ravages of tropical disease attract attention and afford scope for research work. The life history of peculiar helminths such as *Ternidens deminutus* and *Physaloptera caucasica* still await solution. Bilharziasis of both forms exist and *B. hæmatobia* especially is a matter of importance. We may note that plague has become a major public health problem and is spread by the rodents of the veld so that its prevention has become essentially a zoological study.

In Johannesburg is situated the South African Institute of Medical Research and in Capetown there are the Wernher and Beit Medical Laboratories of the University.

Tanganyika Territory.—The most recent estimate of the population of the Tanganyika Territory places it at 5.000,000 natives and 1,600 European officials.

Medical research is served by the medical laboratory at Dar-es-Salaam in an old building erected by the Germans. A Malaria Research Unit was established in 1931 at the instance of Colonel S. P. James and is adorned by an antimalaria engineer. As in other African countries, trypanosomiasis constitutes a major problem and it appears that *T. rhodesiense* is widespread in the territory. Much useful work has been done by Maclean and others in the treatment of this infection with Bayer 205.

Kenya.—Kenya Colony and Protectorate differs considerably from other African territories we are considering because, thanks to its magnificent mountainous configuration, it is to a great extent a white man's country. The native African population numbers three million, or less than one-sixth that of Nigeria, but there are 17,000 Europeans and 56,000 Asiatics and Arabs.

The suitability of the climate for Europeans is shown by the fact that the European birth-rate in Nairobi is 31·6 per thousand and the death-rate 16·2. The chief scourge to progress from the British viewpoint is undoubtedly malaria and its attendant blackwater fever with its customary high death-rate, so that it is not surprising to learn that the subtertian parasite (*P. falciparum*) accounts for 77 % of the total infections. The chief mosquito vectors are the dangerous carriers *Anopheles costalis* and *A. funestus*. The bionomics of malaria are now being closely studied and have also merited the attention of Colonel S. P. James, as it is obvious that in this direction its suppression lies. The story of the remarkable development of the medical services in Kenya from 1909 to 1933 makes good reading and will be found in a story written by Dr. J. L. Gilks in the *East African Medical Journal* for 1933. There is a special laboratory division which is centred at the Medical Research Laboratory in Nairobi and a great deal of routine work is performed there. There are smaller clinical laboratories in the main centres and an important station at the Mombasa native hospital.

Plague in a country so admirably adapted for rodent life has also become a public health problem and it has become so specialized as to merit the attention of a whole-time zoologist. Natural infection of wild rodents with plague was demonstrated for the first time in 1927 and occurs in the field rat, *Arvicanthus abyssinicus*.

The question of yaws and syphilis is of considerable importance to the Colony. Apparently the two diseases (if indeed they are separable) exist in many districts side by side; so that there is a fertile field for further inquiry. Many districts in Kenya afford a good opportunity for comparing the end-results of bismuth and salvarsan treatment. In Kenya, at the original suggestion of the late Sir Andrew Balfour, the bismuth salts are made locally under the name of sobita and are largely used. The saving to the Government is enormous as each dose of bismuth now costs less than one farthing. On the whole the results of bismuth therapy in yaws has been encouraging, but many more injections are required to produce the same clinical results as compared with neosalvarsan. Stomatitis and abscess formation have been noted and the Wassermann and Kahn reactions have been compared, and on the whole the latter is much the more sensitive.

Uganda.—In close geographical and political intimacy to Kenya is situated the Uganda Protectorate, where the native population is equal to that of Kenya (3,500,000), though the European population is considerably less and numbers 590. Uganda is a Colony of particular interest to students of tropical medicine for here some thirty-one years ago the causal connexion between sleeping sickness and trypanosomiasis was settled by the Royal Society's Commission under the late Sir David Bruce, and Uganda has remained the centre of trypanosomiasis research ever since. At Entebbe is situated the Trypanosomiasis Institute under Dr. H. L. Duke, and naturally under his able guidance has become the world centre for dealing with the the practical aspects of trypanosomiasis. Entebbe was chosen as the meeting-place of the Sleeping Sickness Commission of the League of Nations. Dr. Duke and his collaborators are now engaged upon heroic experiments involving the inoculation

of human volunteers with *Trypanosoma brucei* and *T. rhodesiense* and the prophylactic experiments with Bayer 205 which have recently been published and which promise well.

At Kampala, the capital, the clinical laboratory provides the hospital with a clinical pathological service; there is a second laboratory at Mulago in connection with the native hospital, and here is situated the Medical School of Uganda for training native medical students. The course, which lasts four years, is intended to fit these assistants to recognize and treat the diseases of the country intelligently. As a point of medical interest and a basis for future research, pellagra was noted in Uganda for the first time in 1928, and probably this disease is widespread in Central Africa at certain times, as first recognized by Stannus in Nyasaland.

Northern Rhodesia.—The Northern part of Rhodesia is essentially a tropical country and is of particular importance to the British Commonwealth on account of the valuable copper mines which it contains. The native population is scanty and in this sparsely populated country is estimated at 1,372,000. The Europeans, however, number 13,400, which is the second largest white population in Central Africa, and this is mainly accounted for by the mining centres of Broken Hill, Mufulira, Roan Antelope and N'kana.

There are seven European hospitals, the chief being at Livingstone and Broken Hill; the latter, as well as the Roan Antelope Mine, are provided with an efficient laboratory service in which a considerable amount of interesting work of a pioneer nature has been performed; thus the disease originally and euphoniously known as "Roanitis" has been recognized as enteric and due to *B. typhosus*. Luckily plague is absent, but malaria and tuberculosis are both prevalent.

The home of Rhodesiense sleeping-sickness is the Loangwa Valley (which is part of the Great Rift Valley) which stretches diagonally across the northern part of the territory. From time to time a case of this particularly virulent infection is reported and finds its way to this country for treatment; but the immediate and apparently efficacious effects of Bayer 205 have been most gratifying. Even amongst the native population this trypanosome is not widely distributed, but occurs in an extraordinary spasmodic manner and, according to Kinghorn, it is to be found in about 3 to 4 per thousand of the native population.

The Sudan.—The Sudan has long been recognized as a centre for fertile research in tropical medicine. The traditions set up by that remarkable personality, Sir Andrew Balfour, in the Wellcome Tropical Research Laboratories in Khartoum in the early days of the present century have been nobly maintained by another self-sacrificing Scot—Sir Robert Archibald—and under his direction these laboratories are maintaining a high standard of research. This desideratum is facilitated by the provision of a floating laboratory, presented by Sir Henry Wellcome, and by a railway laboratory saloon-car recently constructed for the present director, so that by river and rail the laboratory staff can travel wide and far for the study of local problems.

The medical problems which assail the Sudan are somewhat similar to those in other parts of Central Africa. Bilharziasis here becomes a major problem and its prevention is a matter of major concern, but the discovery by Archibald of a prophylactic tree in *Balanites ægyptiaca*—whose berries, kernels, bark, roots and branches, like some gorgon-headed hydra, contain an active principle which is deadly to the freshwater molluscs, the miracidia and the cercaria—offers a new hope. Practical researches are being carried out on these lines and are being amplified to include guinea-worm infection. A yellow-fever survey of the whole of the Sudan is now being carried out, and a determined effort is being made to compare the differential pathology (if indeed there is any) between syphilis and yaws. At the Kitchener School of Medicine in the Gordon College at Khartoum some thirty Sudanese students are being trained, as we have already noted in Uganda, in the elements of

medicine, and they take a course of training which extends over four years. All who pass the final examination are then posted to large hospitals as house-physicians or house-surgeons, for a year on probation.

Ceylon.—Leaving Africa, our survey leads us to the beautiful and fertile island of Ceylon, that "jewel upon the brow of Asia" which has always offered such a rich field for research. The population, which is somewhat larger than that of Ireland, is estimated at 5,325,000 with some 9,000 Europeans, engaged mostly in the planting industry. Colombo possesses the de Soysa Bacteriological Institute which is at present under the charge of Dr. Lucius Nicholls and has been the seat of a considerable amount of research. In two years' time, however, this ancient and honourable structure is to be replaced by a more capacious and modern building which, with other buildings, will constitute an Institute of Medical Sciences. It is to contain class rooms and special facilities for instruction in parasitology. Ceylon has been a medical teaching centre for nearly fifty years. Most of the medical officers in the Island have obtained their qualifications in the Ceylon Medical College and many, most praiseworthily, proceed overseas to Europe to obtain additional qualifications. The Colombo General Hospital is served by a pathological laboratory devoted to routine work, and the important nature of this service is shown by the recent appointment of Professor E. K. Wolff, formerly Professor of Pathology in Berlin, as a director of this post. There are also two special laboratories, one for malaria and entomology and the second concerned with the ankylostomiasis campaign. Mass treatment of this disease has now been undertaken for fifteen years, and though over 1,500,000 treatments are administered annually, the disease has not yet been stamped out. Under the Colombo Municipality is the City Microbiological Laboratory in charge of Dr. Fabian Hirst, whose work on plague has gained him international recognition. The transmission of this disease through the fleas *X. cheopis* and *X. astia* has been carried through to a successful conclusion. There is, indeed, ample scope for further research work on sprue in Ceylon, and this subject is now engaging the virile energies of Dr. Hirst. Dr H. F. Carter has done very reliable, minute and careful work on the transmission and prevention of malaria in the Island, and from his recently published report it is clear that filariasis offers a fertile problem for further work. The main parasite has been found to be *Filaria malayi* (Brug) and not, as was hitherto thought, *F. bancrofti*, and the transmission through *Mansonioides annulifera* has been traced. The very peculiar habit of Mansonioides of adhering to the rootlets of the water-plant *Pistia* indicates that measures to control the infection should be arrived at by removal of this plant from tanks, irrigation channels, etc. It appears, therefore, that in Ceylon *F. bancrofti* and *F. malayi* exist side by side. The Ceylon Quarantine Board keeps an ever-open and watchful eye upon the possible introduction of cholera into Ceylon from India, and for this purpose maintains a quarantine camp at Mandapam in S. India. Over 100,000 passing through the camp to Ceylon have been investigated up to date and 0·7 per thousand found to be carriers of agglutinable vibrios. So far these praiseworthy efforts have been crowned with success.

Ceylon produces its own *Ceylon Journal of Science* for the publication of scientific papers.

India.—A mere outline of medical activities in India would fill a fair-sized volume, for India is a continent in itself, and has so long been incorporated in the British Empire that its medical institutions are of long-standing and are impregnated with tradition. During the last quarter of a century a fresh stimulus has been imparted to research in the magnificent field in which so many conquests have been won by the Indian Medical Service, and the driving power has to a great extent been imparted by Sir Leonard Rogers. India possesses six magnificent research Institutes—the Central Research Institute at Kasauli, the Haffkine Institute, Bombay, the Pasteur Institutes at Shillong, Coonoor, and the King Institute, Madras,

which are maintained by the Indian Research Fund Association. In addition to these there is the Calcutta School of Tropical Medicine with the Carmichael Hospital attached, and the Institute of Hygiene in the immediate vicinity.

The India Research Fund Association was constituted in 1906, and is controlled by a Governing Body, which in turn is advised by a Scientific Advisory Board, and there is a further technical expert Committee composed of the heads of laboratories in India.

Originally the Government of India gave 5 lahks of rupees (£37,000) annually, but in 1932-1933 the expenditure was £50,000.

The Director of Research has, since 1922, been assisted by an annual conference of research workers for the whole of India. At this conference the programme of work is mapped out for the next year, and there are various sub-committees dealing with special diseases, who make recommendations to the Research Fund Association. The Members of the latter body are the Director-General I.M.S., the Public Health Commissioners, the Director of the Central Research Institute and of the School of Tropical Medicine, Calcutta. At this School there are four separate research posts financed chiefly by the large European trade associations of India.

Sir Robert McCarrison is engaged on nutritional work at Coonoor in an institute situated in ideal surroundings with ideal opportunities, and even the enormous field he has covered appears to have merely touched the fringe of this great and vital subject. The problems with which Research in India is faced are legion. There is the kala-azar transmission problem, but the main outstanding question is, as always, the stamping out of cholera. The malaria problem is yearly being cleared up through the Malaria Survey of India, by the energy and originality of Lieutenant-Colonel J. A. Sinton, V.C., and Lieutenant-Colonel Covell. This Survey necessarily has become a searching investigation of individual localities; while plague and its many attendant problems is ever present.

The mouthpiece of medical research in India is, of course, the *Indian Journal of Medical Research*, which since 1912 has replaced the *Scientific Memoirs* of the Government of India. It is a quarterly journal and in special numbers are issued the Records of the Malaria Survey of India and, since 1924, special numbers appear from time to time dealing with special subjects.

Malaya.—The medical centre in the Federated Malay States is the Institute for Medical Research, Kuala Lumpur, and the fine new building which was completed in 1928 stands upon the site of the original laboratory, made famous by the researches of Sir Thomas Stanton, Fraser, Braddon and others on the ætiology of beriberi, and so may justly be regarded as the birthplace of the vitamins. It consists of five large laboratories, each some 800 feet square, and five smaller rooms. Adjacent to the building is the experimental-animal house, while the whole compound extends over 25 acres, so that a large number of animals can be kept. Therefore here we have an ideally situated research centre in close touch with hospital practice, with its finger on the pulse of medicine, and with its private park and Zoological Gardens. This Institute issues from time to time a series of studies which have received world-wide recognition. One need hardly mention those of Fraser and Stanton on *Beriberi* and on *Melioidosis*, Fletcher and Jepps on the *Dysenteries*, Green on *Malaria* and Anigstein on *Tropical Typhus*. The present Director, Dr. A. Neave Kingsbury, following the lead given by W. Fletcher, is doing admirable work on the ætiology of tropical typhus, especially with regard to prophylaxis, and attempts are now being made by the Director, in collaboration with Hodgkin, to prepare a vaccine from the ticks which act as intermediate hosts.

The question of typhus has become a very large subject. In Malaya two types, the rural (K type) and the urban (W type) claim attention. The former is mite-borne, and its identity with Japanese mite-fever now appears to be established. The latter urban type is flea-borne. Whether, eventually, the louse-borne, tick-

borne, and flea-borne strains of this disease will prove to be merely individual mutations remains to be seen. Dr. R. Green is performing the most valuable work on malaria, and has done great service by scientifically comparing the various newer methods of treatment of this scourge. He has also transmitted the monkey malaria (*Plasmodium innui*) through anopheles, and has established this form of malaria as a new field for research on malaria as a whole. A division of chemistry is dealing especially with the vitamin content of rice. A vast amount of routine work is carried out in this remarkable Institute, amounting to over 20,000 examinations in the year and including nearly 1,000 autopsies, for in close proximity is situated the district hospital with 750 beds, and the Malay Hospital with over 100. In addition, the General Hospital, two and a half miles away, and the European Hospital, five miles away, provide another 400 beds. With goodwill and coöperation everything is possible and this is at it should be.

Straits Settlements.—The Straits Settlements are graced by the great township of Singapore, and here is situated, as a medical centre, the King Edward VII College of Medicine, of which the Professor of Medicine is that sound physician, Dr. R. B. Hawes. The population was estimated in 1931 as about 1,168,000, with some 10,000 Europeans (0·9%). There are also Eurasians, Chinese, Malays and Indians.

The Medical School caters for a class of over 100 students and the standard fulfils the requirements of the General Medical Council of Great Britain. There is also a Dental School attached, which is in fact the only one in the British Crown Colonies.

Singapore is a magnificent centre for morbid anatomy and pathology and affords excellent opportunities for further research. There are pathological centres too in the laboratories at Penang and Malacca. At Penang, for instance, the pathologist reports upon a list of over 2,300 autopsies in the year. Of important investigations carried out here are those on diet and metabolism of Asiatics by D. J. A. Campbell, and the pathological studies of Dr. G. A. Finlayson. Research in bacteriology is being directed to a study of the leprosy bacillus and to the leptospira group.

A matter of human and æsthetic interest has been the recognition of a form of lead-poisoning by Dr. Hawes amongst young Chinese girls who had been powdering their noses with a white powder containing a high percentage of white lead.

Hong Kong.—Hong Kong is an outpost of British Medical enterprise in China for it is now the proud possessor of a Medical College and a University. This was founded, as is now universally recognized, at the instance of Sir Patrick Manson in 1887, and constituted his first venture into the sphere of medical education. The University as now constituted was opened in 1912 and comprises the three faculties of medicine, engineering and the arts, and the Hong Kong College of Medicine founded by Manson, became the Faculty of Medicine in the new University. Hong Kong in its many European and Chinese hospitals provides excellent facilities for medical education, and a number of Chinese medical students are now educated over a five-year course in accordance with the curriculum laid down by the General Medical Council. Here is also a bacteriological institute and a chemical laboratory. Hong Kong is remarkable for the large amount of pathological material which affords opportunities for pathological research on a large scale. The medical faculty of the University of Hong Kong publish a medical periodical known as the *Caduceus*.

Fiji and the Western Pacific.—Of the scattered possessions in the Pacific, the Fiji Islands form the natural centre, and Suva, the capital, the nucleus of medical activity in this wide area. The beautiful islands are no longer the happy hunting grounds of cannibal gangs, but shelter a well-ordered and civilized native community.

I often wonder whether the shade of Thakombau, the last and greatest of Fijian chiefs, gazes with wonderment upon the native Central Medical School. Tuition in medicine has been in existence for over forty years and now, since 1926, the administration of Tonga, Samoa, Gilbert, and Ellice Islands, the Solomons and the

New Hebrides have agreed to contribute towards its upkeep, and the Rockefeller Foundation has here again rendered generous aid. The course of study extends over four years, and facilities for instruction in maternity work are forthcoming. There are now about forty students and post-graduate courses have been given.

A native *Medical Practitioners' Journal* is also in existence. The native practitioners' service now consists of 59 members (including five Indians). It is gratifying to learn that the School is turning out a good type of native doctor—keen, loyal and self-reliant, and these good fellows are in much demand throughout the lonely islands of this great ocean, and everywhere their services are well spoken of.

There is a fine Colonial War Memorial Hospital at Suva, and a bacteriological laboratory. The chief disease is bacillary dysentery, on which still much research work is required, especially as regards antiserum treatment, and there is nowhere where this work can be more advantageously carried out. In Fiji in recent years there have been outbreaks of epidemic dropsy, a disease of which the ætiology is still obscure. Leprosy is a serious problem and there is a fine leprosarium on the Island of Mokagai, where ample opportunities exist for therapeutic adventures in this fell disease.

In filariasis of the Pacific there lies still unsolved a medico-zoological problem of the first magnitude. What particular factor underlies this phenomenon of periodicity? Is it a zoological or a physiological problem? If there are two closely similar periodic species of filaria in *F. bancrofti* and *F. malayi* why should there not exist a third *non-periodic* species peculiar to the Pacific?

British Guiana.—British possessions in the Western Hemisphere have been in close association with medical progress since the glorious Elizabethan days. In British Guiana—a very ancient colony—there remains still much work to be done. In Georgetown there is a fine hospital and a pathological laboratory. Nephritis is remarkably common in this colony, and 1,000 deaths are registered annually as due to this cause. There is reason to believe that a proportion at any rate is due to the prevalence of quartan malaria, whilst others are possibly œdema due to a vitamin deficiency.

British Guiana also possesses a medical novelty in the shape of paratyphoid C. fever, which was discovered by Giglioli in 1926. It has been called "intestinal fever," but intestinal symptoms are, curiously, often absent. The organism can be isolated from the blood and spleen post mortem, and also from the urine. It may also be obtained from fixation abscesses.

West Indies.—We now approach the end of our ambit of the globe by a brief reference to research activities and opportunities in the West Indies. The Caribbean Sea, since the Middle Ages, has been the scene of golden opportunities and British enterprise. It was in St. Kitts, 1764, that James Grainger, a practising physician, originally recognized two fundamentally distinct types of dysentery, and in this manner anticipated the work of the East India physicians by some forty years. It was in Barbadoes too, in 1766, that Hillary first recognized and described the disease we now know as sprue.

Jamaica possesses a pathological laboratory whence, in the years preceding the Great War, emanated many authoritative papers from the pen of Dr. Harold Scott. The particularly careful nature of his work has been generally recognized. His clinical, pathological and bacteriological studies were all of them first-class. Here he described the diseases known as central neuritis of Jamaica and cleared up the mystery of the "vomiting sickness," which he proved to be "ackee poisoning."

Trinidad and Tobago.—These Islands possess a very virile research department in the Bacteriological Laboratory, originally built in 1908, and situated close to the Colonial Hospital in Port of Spain. In 1926-1927 extensive improvements were made, and a library and museum added. Dr. J. L. Pawan, who is at present in

charge, is possessed of phenomenal scientific energy and has produced some very good work. A medical reorganization committee has just been formed with help to extend medical activities in these islands.

The chief research carried out has been the admirable work by Dr. Pawan on what was known as "acute ascending transverse myelitis." Four cases were observed in 1931, and 21 have been recorded up to the present. This disease has definitely been shown to be rabies, which invariably proves to be fatal. Cattle are also affected, and the virus has been transmitted to monkeys.

A considerable amount of evidence has been accumulated to show that the vampire bat *Desmodus rufus*, which in S. America and in the Antilles feeds on the blood of man and cattle, serves as a vector in the spread of this disease.

Much useful work has been done in Trinidad on dietetic questions. Dr. Clark has found that the common native diet of polished rice, white flour, sugar, lard, salt fish or salt meat, though it will support life, is deficient in vitamins.

In addition to those already mentioned the following local journals are devoted to the publication of scientific papers:—

(1) The *West African Medical Journal* (1927).
(9) The *Kenya Medical Journal* (1923), now the *East African Medical Journal.*
(3) The *Malayan Medical Journal* (1926).
(4) The *British Guiana Medical Annual* (1888).
(5) The Port of Spain (Trinidad) Medical Society produces its own journal which is now known as the *West Indian Medical Journal.*

Surveying this vast field as a whole, it is difficult to escape the conviction that the day is rapidly approaching when some centrally constituted body will have to be formed to direct and coördinate these many scientific affluents into one parental stream. There are the opportunities, there are still scientific diamonds waiting to be picked up! We want the young men to step forward to grasp these opportunities, for we refuse to believe that this country is as lacking in enterprise and initiative as ever in her history. A serious attempt to correlate research and medical endeavour as far as the Colonies, Protectorates and Mandated Territories are concerned, was made by the Colonial Medical Research Committee which issued three magnificent memoranda under the editorship of Dr. H. Harold Scott, and it is from these pages that many of the facts related in this paper have been gleaned. Unfortunately this enterprise was stopped in 1931, but it is felt that research within the tropics has now become part and parcel of Medical Research as a whole. There is no dividing line between "tropical medicine" and general medical research; these two are so interlocked and intertwined that he who wishes to advance the cause of tropical medicine must necessarily keep abreast of all the main advances and researches in medicine as a whole. Too narrow specialism in tropical medicine is not to be encouraged as damaging the interests of the subject as a whole—your complete tropical physician must necessarily be a good physician first with enhanced powers and broader outlook in differential diagnosis and in the realm of treatment. There is room for some considered policy which will bring research on specialized subjects such as metabolism, dietetics, zymotic diseases in the tropics, the various blood diseases (pernicious anæmia of pregnancy and sickle-cell anæmia) into line with research work in this country.

There is no lack of opportunity for instruction in the principles of investigation of tropical disease. In Great Britain there are four centres where this subject is taught. In only two of them, however, are clinical facilities—which are all important—at all commensurate to their needs.

In Aberdeen instruction is given in tropical medicine and hygiene, but no diploma is awarded; in Edinburgh courses are given and a diploma of D.T.M. & H. awarded by the University. The course is six months. In Liverpool the course of instruction

is three months, and, perhaps wisely, two diplomas, D.T.M. and D.T.H., are awarded. Facilities for clinical instruction exist.

In London the course lasts five months at the London School of Hygiene and Tropical Medicine and there are two degrees to aim at, the M.D. in Tropical Medicine in London University, and the D.T.M. & H. of the Conjoint Board. As is well known, special facilities for clinical instruction exist in London, and it is the urgent hope of all concerned that these may be considerably extended in the future. The underlying aim of instruction in tropical medicine should be directed towards the treatment and cure of disease, and some agreement should be reached, by consultation, regarding the standards required in the various centres imparting the knowledge of tropical diseases. The time has surely come when the outstanding claims of clinical tropical medicine should be admitted on the same level as those of the subjects of protozoology, helminthology, and entomology, to which it has given birth, and when it should be recognized that scientific nomenclature is only a bye-product of this essential and progressive department of medicine.

It is necessary that one should define what is meant by "clinical tropical medicine"; by this term is to be understood the clinical phenomena presented by disease in the tropics, its differential diagnosis and treatment; and—most important—the recognition of signs and symptoms in different native races. This last is still an almost unexplored field. The divergent reactions to stimuli which they exhibit and a due estimate of their significance can only be obtained by a long and intimate study.

The hospitals for tropical diseases must be in close proximity to the schools of instruction. They should be incorporated in the same block of buildings, as has been done in Hamburg and in Calcutta. We have seen the mischief which results from the too wide separation of the laboratory from the clinical side. And it is probably along these lines that further progress will be made.

Section of Odontology

President—FRANK COLEMAN, M.C., L.R.C.P., M.R.C.S., L.D.S.E.

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PRESIDENT'S ADDRESS

By FRANK COLEMAN, M.C., L.R.C.P., M.R.C.S., L.D.S.E.

THE Odontological Society of London emanated from the deliberations of eighteen dental practitioners who first memorialized the Royal College of Surgeons of England to take action in the direction of professional reform by organizing a dental department in their College, and while this important subject was being considered by the Council of the College of Surgeons these same gentlemen, later known as the Memorialists, met at each others' houses as occasion offered and proceeded to form a scientific society bearing the above name. About the same period, another society, known as the College of Dentists came into existence, after passing through a somewhat similar preliminary stage before being constituted into a society. Briefly, the policy of the College of Dentists, entailed a separation from, and that of the Odontological Society a union with, General Surgery.

At the close of the year 1856 both of these societies had been formed and each held its inaugural meeting early in the following year at the same rooms in George Street, Hanover Square. There was much controversy on both sides as to which society would best fulfil the wants of the profession, and for a time discussions were both acrimonious and discourteous between these two bodies. Later, however, when representatives of these two societies met under less restrained conditions and discussed their respective merits and demerits, it gradually became apparent that their differences and difficulties were not insuperable and might lend themselves to adjustment; both were aiming for the same goal, but in a different manner.

The Odontological Society of London, although a smaller body numerically, included the more prominent and influential members of the profession and eventually prevailed with its broader and more progressive outlook. The more enlightened members from the opposing camp gradually became absorbed into the Odontological Society of London, and on May 4, 1863, an amalgamation of the two Societies was formed under the title of The Odontological Society of Great Britain and under the leadership of Samuel Cartwright (junior). The College of Dentists which had led an independent existence for five years thus ceased to exist.

Previous to this amalgamation each society had its own hospital and school, and each society published its own transactions. The Dental Hospital of London (later Royal) was founded by the Odontological Society of London; whereas, the Metropolitan School of Dental Science, and two years later the National Dental Hospital, was founded by the College of Dentists. The *British Journal of Dental Science*, originally devoted to the interests of both parties, swayed over in its views and eventually became the official organ of the Odontological Society of London, and the *Quarterly Journal of Dental Science*, later replaced by the *Dental Review*, represented the views of the College of Dentists. At least three journals with a sporadic existence of a few months had appeared previous to these, the earliest

dating from 1843. The name of this society has thus changed on three occasions; its housing has changed on several occasions, but in each case, and undoubtedly so on the last occasion, for some good reason. The only factor which has remained constant is the day of the week on which we hold our meetings.

The history of the Odontological Society is so much wrapped up with that of dentistry as a whole that it has been said to be the history of the emancipation of our profession and to the efforts of its members are due our recognition by the Royal College of Surgeons of England and the founding of the first dental hospital in London. The history of the Odontological Society, the College of Dentists, and the Dental profession in general, has been clearly and concisely set out in Alfred Hill's book, entitled "Reform in the Dental Profession."

I have often thought we should attain a truer conception of the teeth with regard to their pathology and their treatment if we gave more attention to the soft parts of which they are composed and less attention to the calcified matrix or bone in which these soft parts are lodged. The calcified structures of the body, whether teeth or bone, are almost passive structures with regard to the part they play in physiology, for the calcified structures react directly neither to nervous impulses nor to alterations in the blood-stream, both of which factors largely determine and control tissue metabolism. If the X-rays showed the soft tissues rather than the hard tissues, I think our pathology and treatment would be brought more into alignment with pathology and treatment elsewhere in the body.

Between the fifth week of intra-uterine life and the fifth year of the child's life, the enamel organ and the dentine papilla of the various teeth commence to form; the earlier period being that of a deciduous incisor tooth and the later period that of a third molar tooth. The dental papilla may be regarded embryologically as a subcutaneous or dermal papilla, and it contains all the structures found in a subcutaneous or dermal papilla, truly in an embryonic form at the earlier date, but in an adult form at the later date, i.e. arteries, veins, nerves, lymphatics, cellular and fibrous tissue. Now, it seems reasonable to assume that these tissues persist in the dental papilla and, thereby, become part of the constituents of the dental pulp. Yet for many years it was doubted whether the dental pulp contained lymphatics, but as lymphatics are present already in this subcutaneous or dermal papilla we might reasonably have assumed that they were present in the later, as in the earlier, stages of tooth formation. The fact that the peripheral cells of the dental papilla undergo calcification does not seem to necessitate a modification in the constituent cells of the enclosed dental pulp, apart from the fact that all tissues subject to metabolism require lymphatics or their analogue for the nutritive exchanges between the blood capillaries and the tissue cells.

Reasoning from the same premises, which appear to be logical, need we have doubted whether the pulp contained sympathetic nerve-fibres? for these also were present at an earlier stage of tooth development when the pulp was represented as a dermal papilla. Most of the arteries of the body, with few exceptions, carry with them their vasomotor or sympathetic nerve supply, and the arteries supplying the jaws and teeth form no exception to this general rule. We ask again, why should the formation of a cap of calcified tissue over the dental papilla or later tooth pulp lead to a deprivation or withdrawal of this sympathetic innervation? The arteries permeating bone are not deprived of their sympathetic innervation because of the incidence of calcification.

Our clinical experience affords evidence that the pulp contains vasomotor or constrictor nerve-fibres, for we know that cold relieves the earlier stages of pulpitis by causing a contraction of the small arteries, and we can directly observe the results of vasoconstriction of the vessels of the pulp and gums when using adrenaline; the action of which drug is practically synonymous with sympathetic stimulation.

It is true that a sensory innervation of dentine and possibly of enamel, and some form of tissue nutrition by means of lymph have now been proved, although I think we might have assumed the presence of both of these agencies in the absence of physiological evidence, or until such evidence was forthcoming.

The enamel of a tooth presents a difficult problem with regard to its physiology, for it is the only ectodermic structure in the body which undergoes calcification; elsewhere calcification and ossification occur in mesodermic tissues. The structure which presents the closest analogy to the enamel of teeth, both in its development and structure, is the lens of the eye, and if the fibres or prisms of the lens underwent calcification, a structure very similar to that of enamel would be formed. Even so I think we get a glimpse in the anatomy and physiology of the lens which may be helpful in elucidating some of the anatomical and physiological problems of enamel.

I think we must concede that enamel is a living tissue and therefore contains a certain amount of organic matter, even if this be very small in amount and difficult to detect either by chemical or by histological methods. If non-vital, at what stage or period of its development does it become a non-vital structure, and where else in the body is there this strange anomaly of a non-vital structure among living tissues?

The embryological development of our bodies is carried on in such a standardized pattern or manner as to become almost monotonous in the repetition of its various phases. Growth or absorption of three primitive types of tissue, viz.: ectoderm, mesoderm and entoderm, their union or non-union, their folding and unfolding into more or less complicated patterns and their invagination and evagination into one another bring about most of the changes from the ovum up to birth. An increase of cell-growth occurs where a solid viscus, a muscle, a tooth, or some other structure, is to be formed; a diminution or resolution of cell-growth occurs where a serous sac, a hollow viscus, a cheek sulcus or a duct is to be formed, these two processes being known in histology as proliferation and absorption.

The various stages in the development of glands, muscles, bones, connective tissues, the solid and hollow viscera are uniform and similar from wherever they arise.

The formation of a tooth, apart from its calcification, is really only a modification of a process which Nature employs when forming a gland, substituting entoderm for ectoderm in the case of a gland opening into the alimentary canal.

In the case of a tooth, the mesodermal dental papilla represents the subcutaneous or submucous mesodermic papilla forming the parenchyma of the gland and the ectodermic enamel organ represents the ectoderm or entoderm as the case may be forming the duct and acini of the gland. Carrying this analogy a little further, the permanent molar teeth in losing contact with the surface ectoderm resemble somewhat closely in their development that of ductless glands which also have lost contact with their surface ectoderm or entoderm.

The recent work of Professor J. E. Frazer gives greater prominence to the part taken in tooth formation by the mesoderm which he states grows up and cleaves the labiodental lamina and subsequently isolates it into its two strands known as the labial lamina and the dental lamina. The process, therefore, as Keith remarks, is more of the nature of a mesoplastic invasion into the ectoderm than an ectodermic invasion into the mesoderm.

With such uniformity in development and design one cannot help thinking that there must be a similar uniformity in the physiology of our tissues. The work of Mummery and Fish on the innervation, and on the nutrition of tooth structure respectively, tends to bring these parts into line with physiology as a whole.

There has been a tendency in the past to create a physiology and a pathology in

connexion with the teeth which is at variance with that which occurs in other parts of the body, consequently our treatment has lagged behind and not infrequently has been inconsistent with present-day views. I think this is partly due to the fact that we allow the calcification of ectoderm and of mesoderm in tooth formation to obscure our views of the physiology of the soft tissues. The physiology of the connective tissues elsewhere is not greatly modified because some become calcified to form bone.

Another subject which has interested me relates to teeth which remain unerupted but yet fully formed. In some cases these teeth, more especially when malplaced, appear to be devoid of a periodontal membrane, and consequently of a socket. It seems that Nature does not proceed to form a socket in those cases where there is no likelihood of a tooth erupting or if erupting of being of service to the individual.

I have observed for many years, and I think correctly, that aberrant buried teeth are not infrequently devoid of a periodontal membrane so that the surrounding bone and tooth substance are in intimate contact, so much so that it is difficult to separate the one from the other in attempting to remove the buried tooth and apart from the appearance and sense of touch it may be difficult to tell whether one is dealing with bone or tooth structure.

It seems unlikely that Nature would proceed to the extent of laying down a socket for a tooth which is neither going to erupt nor be functional. I can recall a large number of cases where buried teeth have had to be entirely chiselled out of the bone before they could be raised from the jaw, and I am sure others have had a similar experience. Teeth are sometimes misshapen, inverted, or orientated in a position which would preclude any future usefulness even if they should erupt, and I feel sure that such teeth will be found devoid of sockets and their membranes.

If this surmise is found to be true it may throw some light on the eruption of teeth, for it would then suggest that the mesoderm forming the periodontal membrane and crypt of the tooth plays an important part in eruption, in so much that in its absence tooth eruption remains in abeyance. In this connexion I should like to quote from Sir Arthur Keith ("Human Embryology and Morphology"):—

> "Eruption is a definite growth movement—allied in Nature to the mechanism which leads to the extrusion of a foreign body by the tissues. During the eruption of the tooth there is not only an absorption of the overlying tissues of the gum—probably due to pressure—but there is also the positive growth of the periodontal tissues at the base of the tooth-sac which, as it presses the tooth towards the surface, moulds the surrounding wall of a dental crypt into a suitable alveolar socket. Thus the formation of the socket or alveolus appears to be part of the mechanism of eruption."

It should not be a difficult matter to determine whether a buried tooth always has a periodontal membrane; from my own clinical experience in dealing with buried teeth I should be inclined to express the opinion that in many cases it gives every indication of being absent. I understand that Mr. Bowdler Henry for his Hunterian Lecture at the Royal College of Surgeons is investigating the development of the third molar tooth and for this purpose is examining a large number of sections of jaws between the ages of four and ten years. As far as I am aware such an investigation has not previously been undertaken and I am hopeful that his work will throw light on many problems which at present are obscure.

The formation of a tooth at the fifth year, when the jaw is practically in an adult condition from the point of view of development, must be quite a different process from that which occurs at the fifth week of intra-uterine life when the tissues of the jaw are in an embryonic and plastic condition. At this earlier period the surface ectoderm is represented by one or two layers only of cells instead of the stratified epithelium at the later period; the only cells which survive to this later period are those which form the basement membrane which represents the original

layer of ectoderm lining the primitive buccal cavity. At this earlier period of tooth formation, the jaw or first visceral arch consists of a mass of mesoderm enclosed between the surface ectoderm of the primitive mouth and the entoderm of the primitive foregut; whereas, at the later period (i.e. fifth year) tooth formation is occurring for all practical purposes in adult tissues.

We have not yet been informed by what agency the absorption of the bony jaw to make room for the developing tooth-germ is brought about; in the earlier periods of tooth development the soft tissues are absorbed or pressed aside to make room for the tooth germ, but at the later periods we are dealing with fully formed bone. It is evident, therefore, that the process of development and of eruption of teeth must be different at the fifth year from that which occurs at the earlier periods; yet textbooks describe the process as though it were identical throughout this large range of time, extending almost from the commencement of the development of our body to adult life.

It is a remarkable fact for any organ or structure which serves in the economy of the body to be delayed in its formation until adolescent life. Every other part of our body of functional value in its growth has already been formed many years previous to this, in fact in most cases at or before birth. The ovary and testis have progressed to such an extent at this period that they are ready for reproducing another individual and even as early as the sixth week of intra-uterine life they have actually selected and set aside certain cells for this purpose.

A recent branch of embryology known as experimental embryology, tends to show how one part is dependent upon another, and that one developing tissue can exercise a controlling influence on a neighbouring tissue. For instance, it has been shown that if the developing optic cup be removed or destroyed, the formation of the lens of the eye does not take place, but on the other hand if the developing optic cup be transplanted under ordinary ectoderm, then the cup causes the overlying cells with which it comes into contact to grow towards it and form a lens.

Another interesting fact derived from this field of research shows that ectoderm which would eventually have formed skin, when grafted or implanted amongst ectoderm destined to form brain tissue, is converted into nerve-tissue or vice versa, i.e. ectoderm destined to form nerve-tissue when transplanted into a skin region becomes skin. Experiments at earlier stages of development naturally lead to far more extensive results, even to the formation of an imperfect twin; whereas, injuries to the germ plasm of the ovaries of mice, as by prolonged exposure to X-rays, are capable of bringing about deformities and malformations in their offspring even to the second generation.

Clinical evidence had previously given a clue to these changes in the so-called "implantation-cyst" arising from an injury that displaced a portion of skin into the deeper tissues which resulted in the formation of a skin (ectodermal) tumour and not a connective tissue (mesodermal) tumour. Further evidence was afforded in the developmental dermoid tumour and in the different varieties of skin grafting where a skin surface would take upon itself the character of a mucous membrane or vice versa.

This recent branch of science opens up a wide field for research which may lead to results of benefit to mankind, for if the germ plasm can be influenced adversely may it not be possible to influence it beneficially. These experiments have been rendered possible by our being able to keep fragments of growing and developing tissues alive in artificial media, so that they can be studied in their living state; a method of "tissue culture" as opposed to "cell culture" which has been employed for many years by bacteriologists in the study of living micro-organisms.

In our speciality it may throw some light on the pathology of the composite odontome; it certainly seems to contra-indicate the removal of the lining membrane

in the treatment of a dental cyst, for what better epithelial graft could we have than that which Nature already supplies?

I was privileged recently to take some share in the discussion on the alteration in nomenclature of some of the subjects in the dental curriculum, both at the University of London and at the Royal College of Surgeons of England. We disapproved of terms such as dental physiology, dental bacteriology and dental materia medica for the reason that they were apt to confine their subjects in watertight compartments, and to create in the mind of the student a feeling that these sciences in some abstruse way had a different bearing on and relation to the teeth to what they had elsewhere in the body. The terms adopted to replace those inappropriate ones were special physiology, special bacteriology and special materia medica, using the word special in the sense that the limitation was not in the science concerned but in the region of the body to which it was applied.

Section of Physical Medicine

President—J. Barnes Burt, M.D.

[*October* 19, 1934]

Some Problems in the Study of Sciatica

PRESIDENT'S ADDRESS

By J. Barnes Burt, M.D.

We must all admit that the literature on sciatica is somewhat varied and contradictory, and the information provided does little to assist the medical man in the management of a case. The members of this Section see more cases than all the other Sections put together, and in view of the vagueness of most textbook descriptions and the uncertainties of treatment, I feel that a discussion may help to put the subject on a more practical basis. With the introduction of modern methods of examination our knowledge of sciatica has improved enormously, and the time has come to use the pruning-hook on the mass of dead wood which has inevitably formed around a disease recognized for centuries.

Problem 1.—Classification

Copeman [1] very aptly remarks that to diagnose sciatica is merely to diagnose leg-ache in Latin, and it does not help us very much to divide leg-ache into primary and secondary, or neuritis and neuralgia. Nearly all sciatica is secondary, and neuritis and neuralgia are often stages of the same process. It is far more practical to divide sciatica into three main clinical groups, namely: (1) Root sciatica; (2) trunk sciatica; and (3) referred sciatica. This classification is also an anatomical one, and forms a definite basis for diagnosis and treatment.

The groups are quite distinct, and are in most cases entirely different diseases, with different pathology, onset, and treatment. Thus, root sciatica is most commonly due to intervertebral arthritis or strain of the lumbar vertebræ, and pressure from new growths; trunk sciatica, to inflammation of the sciatic sheath, a condition which may be primary, but is most often due to the spread of inflammation from the gluteus maximus or pyriformis muscle; and referred sciatica, to inflammation in some structure supplied by the sciatic plexus. It is common to say that the last group is not true sciatica. At any rate it is true leg-ache, and the pain is distributed along the branches of the sciatic nerve, and although there is no active inflammation in the sciatic trunk, it is more than likely that a small branch of the nerve which supplies the affected joint or bursa is inflamed.

All these terms have been used before. This is the French classification, with referred sciatica added as a group. Gowers, Llewellyn [2], Thompson and Gordon [3] all used the terms "primary" and "secondary," but the primary and secondary sciatica of Llewellyn and Gowers is quite a different thing from the primary and secondary sciatica of Gordon and Thompson.

Putti [4] has written the classical article on root sciatica, and has established this group as a clinical entity. He states that it is the most frequent type of sciatica. Out of 140 cases of sciatic pain under my care at the Devonshire Royal Hospital in the last three years, 35·5% belonged to the root group, 17% to the trunk group, and 45·5% to the referred group.

With regard to trunk sciatica, nearly all the textbooks dwell on this to the exclusion of other types, and to judge from these articles, this is the commonest group.

Referred sciatica receives very little attention, and is chiefly mentioned in differential diagnosis. It is a definite group, and deserves a place to itself just as much as trunk or root sciatica. The structures supplied by the sciatic plexus, which, when inflamed, may set up pain along the course of the nerve, are (1) the hip-joint; (2) the sacro-iliac joint; (3) the gluteal and trochanteric bursæ; (4) the prostate; (5) the gluteus minimus and gluteus medius, which, though not supplied by the sciatic nerve, are supplied from the fourth and fifth lumbar roots.

Problem 2.—Differential Diagnosis

This is not the place to enter into a long differential diagnosis which can be found in the numerous monographs and textbooks, but it will be useful to discuss the difficulties and contradictions found in the various articles on the subject.

Generally speaking, the symptoms of referred sciatica are less severe than in the first two groups. The pain is seldom constant, and often does not reach below the ankle. Sensory signs are absent, and although there may be some wasting of muscle, this is of the general type due to disuse. Unfortunately, similar symptoms may occur in a mild case of root sciatica, and it is necessary to examine the various structures supplied by the sciatic plexus to make a diagnosis. It is quite a mistake to think that all cases of root sciatica are severe or produce serious symptoms.

Putti regards rigidity of the lumbar spine as a constant sign in root sciatica. There are two other signs of root sciatica, which, though not constant, often help one to a diagnosis: (*a*) Tenderness along the lateral aspect of the lumbar vertebræ; (*b*) distribution of pain along the external cutaneous nerve of the leg (L.3). It is seldom that osteo-arthritis of the fifth lumbar vertebra occurs without other lumbar vertebræ being affected, and the presence of pain over an area supplied by the third lumbar nerve suggests root sciatica.

Problem 3.—The Varieties of Referred Sciatica

Osteo-arthritis of the hip.—This as a cause of referred sciatica is very much exaggerated. The referred pain in this condition is generally along the anterior crural nerve to the knee. When it is along the sciatic nerve the pain is slight, and only mentioned by the patient on being asked a direct question. In only two cases has one come across serious sciatic pain associated with osteo-arthritis of the hip, and on investigation it was found that there was a secondary osteo-arthritis of the spine. One or two observers have suggested that inflammation of the hip-joint may spread directly backwards to the sciatic nerve, and thus cause trunk sciatica. This could only occur in very acute infection of the joint, for the sciatic nerve is separated from the hip-joint by the tendon of the obturator internus.

Sacro-iliac disease.—This is a common cause of referred sciatica, and the diagnosis of the condition is full of problems. In many cases of true trunk sciatica there is pain and tenderness over the sacro-iliac joint, the pain being referred along the small branch of the nerve which passes into the joint. There is no actual inflammation of the joint in these cases. On the other hand, inflammation of the sacro-iliac joint may produce a referred pain along the branches of the sciatic nerve, and only by testing the tenderness of the nerve-trunk can the conditions be separated. Dr. James Mennell has introduced a useful sign in testing the tenderness of the sacro-iliac joint—one must palpate from within out. If the palpation is made from without in, gluteal fibrositis may obscure the diagnosis.

You will remember that the roots of the sciatic nerve pass very close to the anterior margin of the sacro-iliac joint, and certain authorities consider that the inflammation may spread to these roots by direct contact with the joint. This is denied by Garforth and Wilson after careful investigation, and one can understand that if it occurs at all, it must be very rare, for the position is quite unlike that met with in root sciatica. Assuming that there is sufficient swelling of the sacro-iliac joint to press against the roots, there is plenty of room in the pelvis—a very different state of affairs from vertebral sciatica, where the roots lie in the confined space of the intervertebral foramina. The swelling of an intervertebral joint presses the root against the opposite sides of the foramen, and in certain cases the inflammation actually extends into the fibrous tissue around the roots. This explains the great differences in the severity of the symptoms in root sciatica.

It has always been a problem to me why there is no sciatic pain in the early stages of spondylitis ankylosa. Practically in every case the sacro-iliac joint is ankylosed, and one would have thought that the inflammatory changes necessary to produce ankylosis would set up referred pain in the same way as osteo-arthritis or sacro-iliac strain. It is easy to see why spondylitis ankylosa does not produce root sciatica, for the arthritis belongs to the dry type, and there is early immobility; hence no pressure is exercised inside the intervertebral foramina. Another problem is this: The sciatic nerve sends branches to the knee-joint and the ankle-joint; why does not arthritis in these joints set up referred pain along the sciatic nerve?

Sciatica due to gluteal or trochanteric bursitis is not common. In these cases there is no pain above the line of the ischial tuberosity, and diagnosis is based on finding the tenderness strictly localized to the bursæ.

Fibrositis of the gluteus medius and gluteus minimus.—To me this is the most interesting of the referred sciaticas, partly because it is not mentioned in textbooks, and partly because it is easily cured if correctly treated. These muscles, for the most part, lie on bone, and are covered by one of the firmest aponeuroses in the body. A branch of the superior gluteal nerve travels along the surface of the ilium to supply the muscles. Any inflammatory swelling of the muscle in this confined space sets up pressure on the nerve, or possibly the nerve is involved in the inflammation, and the pain is referred to areas of the leg supplied by the fourth and fifth lumbar, chiefly the peroneal branch of the sciatic nerve.

The diagnosis depends on finding definite fibrositis in the gluteal muscle, absence of Lasègue's sign, and absence of tenderness over the trunk of the sciatic nerve. Many cases of sciatica in which there is quick recovery belong to this class.

It is quite possible for this type of referred sciatica to develop into a trunk sciatica. If a patient with fibrositis of the gluteus medius continues to work, or attempts to walk-off the sciatica he may aggravate the condition, and the inflammation will spread from the gluteus medius to the fascia of the gluteus maximus. From there it spreads by direct contact to the sheath of the sciatic nerve.

Problem 4.—Lasègue's Sign

Both Putti [4] and Buckley [5] belittle the value of Lasègue's sign. Putti chiefly deals with root sciatica of the vertebral type, and in this condition Lasègue's test, if carefully performed, is negative unless inflammation has actually spread into the roots of the nerve.

Buckley gives six causes for the presence of Lasègue's sign, namely, sciatic perineuritis, sacro-iliac lesions, osteo-arthritis of the hip, arthritis of the spine, gluteal fibrositis or bursitis. In my experience Lasègue's sign, if the test is carefully carried out, occurs only in the first two conditions. There may be some slight difficulty in flexing the leg in the other four, but with a little care this can be managed.

There are two modifications in Lasègue's sign which may prove extremely useful:—

(*a*) Dr. Mennell's. This sign is useful in separating the two conditions, trunk sciatica and sacro-iliac lesions. The modification consists of flexion and extension of the foot whilst raising the leg with the knee straight. With the foot flexed the leg can be raised higher in trunk sciatica than when the foot is extended, whereas the flexion and extension of the foot makes no difference in sacro-iliac diseases.

(*b*) The floor sign. The patient sits on the floor with the back resting against the wall. This fixes the pelvis and reduces the spasm of the hamstrings. The sign is positive where the back of the knee cannot be made to touch the floor. Excluding cases of arthritis of the knee, the sign is only present in acute trunk sciatica, or where there are adhesions between the sheath and the nerve.

Problem 5.—Treatment

Every type of sciatica requires physical treatment, and it is well to remember Buckley's [5] aphorism: "A textbook of anatomy is a better guide to treatment than a treatise on therapeutics."

There is some uncertainty with regard to the value of manipulation, stretching of the nerve, and the injection of saline and oxygen. I feel that these subjects would be best discussed at a joint meeting of the Section of Orthopædics and this Section.

Massage.—There is much difference of opinion with regard to the value of massage. Some authorities say that it is always harmful, others, like Pemberton [6], say that massage is the agent which accomplishes most. The truth, as usual, lies halfway between the two views. Sciatic pain caused by gluteal fibrositis can be cured by early massage to the affected muscles, but this ought to be followed by at least an hour's rest in bed after each application, and the doctor must definitely state in his prescription "gluteal muscles only," otherwise the masseur devotes much of his time to the painful area, which is situated in the leg, and massage to this part is harmful.

Again, massage is of value in sacro-iliac strain. In all other cases of sciatic pain massage is definitely harmful except in the final stages when the disease is no longer active and wasted muscles require treatment.

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[3] Thomson, F. G. and Gordon, R. G., "Chronic Rheumatic Diseases," Lond., 1926, 44.
[4] Putti, V., *Lancet*, 1927 (ii), 53.
[5] Buckley, W., *Practitioner*, 1930, cxxiv, 653.
[6] Pemberton, R., "Arthritis and Rheumatoid Conditions," Philad., 1929, 312.

Discussion.—Dr. W. J. TURRELL referred to three cases of very acute and sudden pain of sciatic distribution, occasioned by a twisting or semi-rotary movement of the lower spine, such as occurred in driving a golf-ball or wielding a heavy salmon-rod. In these cases the thigh on the affected side was rigidly flexed and adducted. The pain was agonizing, but was immediately and completely removed by manipulation under an anæsthetic. The condition was apparently due to a slight displacement of the sacro-iliac articulation.

Dr. WILFRID EDGECOMBE said that he agreed entirely with Dr. Burt's classification of sciatica on anatomical grounds; it served to simplify ideas on the subject. The group of "referred" sciatica, numbering 45% of cases, related to hospital practice. In private work he thought the proportion of these cases was much higher, approximately 65 or 70%. These cases were easily curable, the treatment being, in the acute stage, absolute rest, local heat, and massive doses of aspirin; in the later stages, intelligent massage following on the application of heat, either dry or moist: and in the chronic stages, persistent massage until all fibrositic thickening had disappeared. The most troublesome cases were those of acute toxic neuritis of the sciatic nerve; the pain was severe and constant, irrespective of movement, and the wasting of muscles was marked and dramatic. The process of recovery was prolonged.

Dr. A. R. NELIGAN said that the President's classification was a good working one but he thought that the diagnosis of "referred sciatica" should seldom be made. It was preferable to attribute such cases to their causes—sacro-iliac conditions for instance. He asked what the diagnostic signs of fibrositis of the gluteus medius were and mentioned cases of sciatica which were not relieved until existing varicose veins were treated. Mennell's sign was most useful and ought to be more widely known. In a recent case of trunk sciatica Lasègue's sign was evident only when the opposite leg was raised. With regard to massage in the treatment of sciatica, the under-water douche was a very useful method.

Dr. ALASTAIR MACGREGOR said that he regarded a great number of these cases as being of the nature of a perineuritis or inflammatory involvement of the nervi nervorum. In cases of subacute and chronic sciatic pain which had resisted all other forms of treatment, he had had great success from the use of galvanic acupuncture, having first localized any tender spots in the course of the nerve-trunk by means of a mild faradic current applied by a small button electrode.

It had been pointed out to him that some anatomical authorities denied the existence of sensory branches of the great sciatic nerve above the bifurcation, which was generally at the level of the knee-joint. If this were so, the faradic test would appear to indicate that referred pain was present in a much greater proportion of the cases than was generally supposed. Whatever the explanation might be, and wherever the source of pain, the fact remained that, if tender points were located by a mild faradic current, treatment by galvanic acupuncture was almost invariably successful.

He had met cases in which the origin of a supposed sciatica had been an enlarged prostate or a chronic appendicitis and in which the sciatic pain had disappeared after appropriate surgical treatment. In none of these cases were there any points tender to faradism, and the tendo-Achillis jerks were always present.

Section of Pathology

President—PAUL FILDES, O.B.E., M.B., F.R.S.

[*October* 16, 1934]

Some Medical and other Aspects of Bacterial Chemistry

PRESIDENT'S ADDRESS

By PAUL FILDES, M.B., F.R.S.

Abstract.—The evolution of bacterial chemistry as applied to medicine from bacterial chemistry as applied for economic purposes is traced. The outlook of the bacterial chemist is contrasted with that of the medical bacteriologist.

The possible results of the study of bacterial nutrition from the medical aspect are noted. Difficulties arise from the fact that the nutrition of a single species varies. A variation in bacterial enzymes results from alteration in the substrate.

The evolution of bacteria from primitive autotrophic types to the parasitic types of the present day may be explained on the basis of an evolution depending upon a gradual alteration in the substrate. The primitive bacteria in an evolutionary sense are endowed with great powers of synthesis, while the evolved bacteria are not. Thus, while the primitive bacteria can synthesize their protoplasm from simple nutrients, the parasitic bacteria cannot and must receive complex compounds pre-formed.

In the case of parasitic bacteria this taking of nutrients from the tissue cells may have a bearing upon disease. An infective disease may in part be a deficiency disease. An essential cell nutrient may be taken from the cells by the bacteria, with a result that they cannot function properly in the "anti" sense.

RÉSUMÉ.—L'auteur trace le développement de la chimie bactérielle appliquée à la médecine depuis la chimie bactérielle appliquée aux besoins économiques. Le point de vue du chimiste bactériologique est comparé à celui du bactériologiste médical.

Les résultats possibles de l'étude de la nutrition bactérielle sont notés. Des difficultés surviennent par le fait que la nutrition d'une seule espèce est variable. Un changement du substratum a comme résultat une variation des enzymes bactérielles.

L'évolution des bactéries du type primitif autotrophique au type parasitique d'aujourd'hui peut être expliquée comme évolution dépendant d'un changement graduel du substratum. Les bactéries primitives dans le sens évolutif possèdent un grand pouvoir de synthèse, tandis que les bactéries évoluées n'en possèdent pas. Ainsi, tandis que les bactéries primitives peuvent former leur protoplasme à l'aide de substances nourrissantes simples, les bactéries parasitaires ne le peuvent pas, et ont besoin de composés complexes déjà formés.

Dans le cas des bactéries parasitaires cette abstraction de substances nourrissantes des tissus a peut-être des rapports avec la maladie. Une maladie infectieuse peut être en partie une maladie de carence. Une substance nourrissante essentielle peut être enlevée de la cellule par les bactéries, avec la conséquence que les cellules ne peuvent pas fonctionner bien dans le sens "anti."

ZUSAMMENFASSUNG.—Die Entwicklung der bakteriellen Chemie in der Medizin aus der zu wirtschaftlichen Zwecken angewandte bakteriellen Chemie ist beschrieben. Der Standpunkt des bakteriellen Chemikers ist dem des medizinischen Bakteriologen entgegengesetzt.

Die möglichen Erfolge der Untersuchungen über bakterielle Ernährung werden erörtert. Aus der Tatsache dass die Ernährung einer einzigen Art verschieden ist entstehen Schwierigkeiten. Veränderung des Substrates bewirkt eine Veränderung der bakteriellen Enzyme.

Die Evolution von Bakterien aus den primitiven, autotrophischen Formen bis auf die heutigen parasitären Formen kann auf Grund einer allmählichen Veränderung des Subtrates erklärt werden. Die primitiven Bakterien, im Sinne der Evolution, haben eine grosse Synthesierungsfähigkeit, während entwickelte Bakterien keine solche besitzen. So können die primitiven Bakterien ihr Protoplasma aus einfachen Nährstoffen zusammensetzen, während die parasitären Bakterien präformierte komplexe Verbindungen verlangen.

In den parasitären Bakterien hat dieses Nehmen von Nährstoffen aus den Gewebszellen vielleicht Beziehungen mit der Krankheit. Eine infektiöse Krankheit könnte auch zum Teil eine Mangelkrankheit sein. Ein notwendiger Zellnahrstoff könnte von den Bakterien aus den Zellen genommen sein, so dass sie nicht richtig in dem "anti" Sinn funktionieren könnten.

THE study of bacteria by the methods of chemistry has for long been a recognized branch of bacteriology. Having its origin with Pasteur in his work on fermentation, it has been applied by Winogradsky, Beijerinck and Omeliansky in the study of autotrophic and soil bacteria, by Harden in the study of fermentation, by Neuberg in that of decomposition, by Meyerhof in that of respiration, and by a host of others of no doubt equal importance but of more recent date.

It will be noted that all these applications of chemistry to bacteriology have been made in branches of bacteriology other than medical, and the reason for this is clear. Pasteur, the first bacterial chemist, was primarily a chemist and was largely interested in the chemical changes induced in a substrate by the growth of bacteria. These chemical changes lead to the production of substances of economic importance. Thus was developed a line of activity in which the microbe was not studied as a living thing, but was merely utilized as an agent for producing chemical effects, and since these effects were often commercially valuable, they tended to retain the interest of chemists in these non-medical activities.

A medical bacteriologist in those days had little interest in the changes which a non-pathogenic microbe could induce in an artificial medium. His interest was centred on those superior microbes which could kill an animal by methods usually obscure, but sometimes more obvious. In the more obvious instances death resulted from a toxin produced by the microbe in a culture medium. Now this toxin was comparable to the alcohol produced by microörganisms from glucose in the experiments of the economic bacterial chemists, and this fact was seized upon as an opportunity to apply the methods of the economic chemists to medicine.

Thus the efforts of the first medical bacterial chemists, Brieger, Sydney Martin and many others, were directed towards finding the chemical nature of toxins, and, as is known, these efforts failed and were abandoned. It was clear that toxins were too unstable and complicated to be capable of chemical analysis at that time.

A further early effort to apply chemistry usefully to medical bacteriology, arose out of the demonstration that some bacteria could be grown on simple media of known chemical constitution—the so-called synthetic media. One of the chief interests of the medical bacteriologist was the isolation and growth of bacteria which were often refractory, and therefore he turned to this field with interest. This interest, however, soon evaporated when he found that none of his really interesting species would respond to these synthetic media, but only, at most, a few coliform organisms, which he treated largely with contempt.

These remarks will explain why bacterial chemistry has been in abeyance in medicine. There is something about the microbes of medical interest which defeats the chemist. A chemist looking at the ranges of bacteria with the idea of applying

his methods in this field, will obviously prefer that range in which he can study the interaction of inorganic materials. He will be appalled by the impossibility of dealing with, for example, ultramicroscopic viruses in terms of chemistry.

However, knowledge in biochemistry is not stationary. It is no part of my function to enumerate the remarkable advances which have been made—how, in the course of a few years, for instance, the nebulous concept of the vitamin has been crystallized literally—how in a still shorter time a cause of tumour growth has been demonstrated and isolated chemically—but to point out that concurrently with these more spectacular achievements, the biochemist—particularly in this country, and especially in the Universities of Cambridge and Leeds—has been exploring the complexities of even the bacteria of medicine. At the present day, a biochemist with his greater biological interests and the knowledge gained by the industrial chemists, when he enters this field is not so much appalled by the chemical aspects of these bacteria as formerly, and feels equipped to enter on the study at any rate of the least exacting of them.

In order to carry out this work satisfactorily, he must clearly work in collaboration with a medical bacteriologist and this has, in the past, been difficult to accomplish. On the one hand, it has been a fact that the great majority of medical bacteriologists have been trained and employed for a specific purpose—the diagnosis and treatment of disease in man. So far as the microbe itself is concerned, he has looked upon it from much the same point of view as does the economic bacteriologist, namely, as a convenient agent for the production of disturbances in animal physiology. The metabolism, for instance, of the microbe has interested him only from the point of view of devising tests for its recognition or convenient methods for its cultivation. He has been essentially a physician, often directly involved with the sick.

On the other hand, the bacterial chemist has been largely a person trained in pure science and he is not at home with the adjustments and opportunism often necessary in medical work. When he learns from the bacteriologist that a particular microbe will only grow upon complex animal preparations, his natural instinct is to inquire what are the chemical substances in these mixtures which make the difference between success and failure. Having obtained this knowledge, he would wish to know what function this substance or state carried out in the microbic physiology and why it is necessary for this microbe, but not for that, to have this function subserved.

Again, when confronted with a case of microbic disease, he is not so much interested, as is the medical bacteriologist, in the lesions found in the animal as in the microbic substances which caused those lesions. He looks upon these disturbances in animal physiology as secondary phenomena resulting from the normal physiological processes of the bacterium, and his inclination is to try to isolate the product of bacterial physiology which is responsible for them.

In general then, the interest of the medical bacteriologist is centred more in the disease produced by the microbe, whereas the interest of the chemist is centred more in the physiology of the microbe.

In order to achieve a satisfactory collaboration between these interests, it would appear necessary for the bacteriologist to abandon a strictly *ad hoc* attitude in his medical work and acquire a greater biological interest, while the chemist or biochemist should extend his interest in the physiology of the microbe to the physiology of the host and to the interactions between these two physiologies which constitute disease. He should have the object ultimately to facilitate or hinder the action of the microbe in this process of disease.

It is not my purpose to review the achievements to date of bacterial chemists, though some of them have great abstract interest and may indeed be capable of direct application to practical medicine.

My own interest, on which I should like to enlarge, is the study of bacterial nutrition.

The object of the medical bacterial chemist in studying the nutrition of bacteria is primarily to find out what are the essential foodstuffs of pathogenic bacteria, and what are the mechanisms by which these foodstuffs are broken down and built up again to form a new individual, in short, as the journalist has said: "what pathogenic germs eat."

He has, however, also in mind the possibility that during the course of this work other matters may claim attention.

(1) He appreciates that in disease the nutrients of a bacterium are the tissues of the host. A bacterium cannot cause disease unless it can grow in the tissues of the host, that is, unless the physical and chemical states of the tissues of the host are suited to the mechanisms of the bacterium in them. If the mechanisms and requirements of the bacterium were known, it might be possible to render the physical and chemical states of the tissues unsuitable for the nutrition of the bacterium, and so to influence disease.

As an illustration of this sort of process may be mentioned the fact that Argyll-Campbell and I have succeeded in influencing the incidence of tetanus in experimental animals inoculated with spores. It had first been shown that *in vitro* tetanus bacilli required a certain degree of reducing intensity in their surroundings before they could germinate. It was then shown that these conditions did not exist in a normal animal and this fact was advanced as a reason for the failure of the spores to germinate. When local areas of high reducing intensity were induced in the animal, then spores germinated in those areas and tetanus occurred. Finally Argyll-Campbell and I showed that animals kept alive in low concentrations of oxygen, so that the normal oxygen tension of the tissues was reduced, succumbed more readily to tetanus, while, conversely, animals kept in high concentrations of oxygen succumbed less readily. Thus an abstract study of the biology of the tetanus bacillus under the artificial conditions of the test-tube, supplied information which could be applied directly to animal pathology.

(2) The bacterial chemist has also in mind the fact that the toxic substances produced by bacteria, and on which some diseases depend, are notoriously subject to increase or diminution in artificial culture, and that this result must depend upon some unrecognized variation in nutrition. Were the details of the nutrition known, the production of toxins could be controlled *in vitro* and, conceivably, *in vivo*.

(3) He also hopes that anything new which may be found in bacterial nutrition may have application in the sphere of animal nutrition.

The extent to which knowledge of the metabolism of bacteria can be applied directly to the nutrition of animals is of course unknown, but there is reason to think that the former idea that bacterial metabolism is a thing apart from the metabolism of plants, protozoa, and animals is no longer tenable.

It is known, for instance, that many bacteria require basic foodstuffs of exactly the same quality as do animals, and that they even require respiratory pigments similar in function and constitution to those of animals. The mechanisms by which these nutrients are metabolized are similar in both cases. The outstanding difference merely depends upon the greater complexity of the animal structure. While the metabolism of a single animal cell may closely resemble that of a single bacterial cell, the conglomeration of cells in an animal to provide services which are outside the interests of a bacterium leads to refinements and complexities of metabolism for which the bacterium has no need.

While, therefore, it may be found that the metabolism of a polymorphonuclear blood-cell is closely analogous to that of a bacterium, it is not to be expected that the metabolism of bone- or nerve-cells will be so close.

Even if we consider such a particularly animal characteristic as the requirement of vitamins, it is not certain that this constitutes an essential difference from bacteria. It has been shown, for instance, that some bacteria synthesize carotene as part of their protoplasm. It is not excreted as a waste product but is part of the cell make-up. It must therefore be assumed that it carries out a function useful to the microbe, that it is a necessary part of the normal bacterial metabolism. In such a case, a study of the function of carotene in a microbe should throw light upon its function in the normal growth of an animal cell.

Further, it is not impossible that substances shown by the bacterial chemist to be necessary for the proper growth of bacteria may subsequently be found to be necessary for the growth of animals. In such work the bacterial chemist has at least one advantage, namely, that he is working with an experimental object which allows a conclusion of the experiment in a few hours, whereas the worker on animal vitamins must wait weeks or months. Further, his experimental object is easily reproducible at will, whereas animal cells are not.

The facility with which bacteria can be manipulated has been recognized already by medical biochemists who are primarily interested in subjects outside bacteriology. For instance, some, at least, of those biochemists who have in view the physiology of the cancer cell find it convenient to study the physiology of the bacterial cell until such time as the cancer cell becomes easier to manipulate.

Such aspirations as these may be in the mind of the bacterial chemist when undertaking the study of the nutrition of medical bacteria, but he does not proceed far before he is confronted by some of the difficulties.

It soon becomes obvious that the essential food requirement of even a particular species of bacterium is not constant. It is a familiar observation that a bacterium when first isolated is much more exacting than a laboratory strain of the same species. For instance, a gonococcus when first cultivated requires a high concentration of unheated serum, but, on subsequent cultivation, the serum may be reduced and finally eliminated. This may be looked upon as a nutritional variation. *The nutrition of a single species is not constant.* I shall now deal with the factors involved in this process.

The nutrition of a cell may be defined as the process by which materials are changed in such a manner as to produce a new living cell or maintain an existing cell. The process thus requires the materials on which the instruments of change may act and the instruments of change themselves—the substrates and the enzymes. The substrates are materials foreign to the cell, while the enzymes are part of the make-up of the living cell itself.

In general, the substrates are of five varieties :—

(1) *Energy-producing.*—These are substrates which, when oxidized or split up by appropriate enzymes, liberate energy which may be measured as heat and expressed as calories. Some of this energy is absorbed and stored again in the process of building-up the proteins and other constituents of the new or old cell by other enzymes, or is expended in external work—motility, &c.

(2) *Hydrogen acceptors.*—These are materials which are capable of being reduced under the influence of the bacterial enzymes and so permit the simultaneous oxidation of the energy-producing material. These materials consist of atmospheric oxygen, oxygen in combination, or reducible substances not necessarily containing oxygen.

(3) *Non-specific foodstuffs.*—These are nitrogen-and carbon-containing materials from which the protein of the microbe is built up by anabolic enzymes with the aid of energy liberated from the energy-producing material (1).

(4) *Specific foodstuffs.*—These are materials which contain chemical structures represented in the make-up of the bacterial protoplasm, but which cannot be synthesized by the enzymes of some bacteria. These must therefore be supplied pre-formed in the diet. Tryptophane is an example of this type.

(5) *Accessory materials* used in the course of the chemical changes involved in (1), (2), (3) and (4). In many cases these accessory materials are elemental substances, such as iron, magnesium, phosphorus and sulphur, which act as catalysts and regulators of the chemical reactions, but in other cases they are more complex bodies, the function of which is unknown. They may be associated with the action of the enzymes (fig. 1).

The enzymes will now be considered. The enzymes are the instruments of change in a living cell. They are organic catalysts which enormously accelerate chemical changes which are chemically possible without them, but which would require an infinite time. They are not themselves living, but an appropriate collection of them congregated in one unit constitutes the difference between a living and a non-living unit.

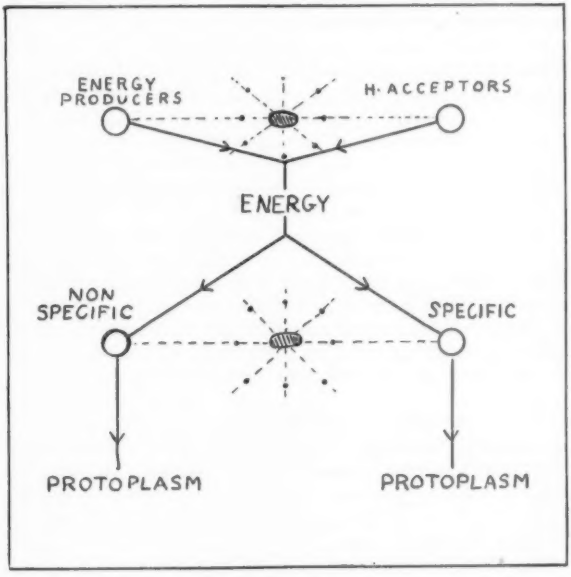

FIG. 1.—The four types of nutrient are shown. At the top, the energy-producing material and the hydrogen-acceptors come under the influence of the katabolic bacterial enzymes and are induced to react and liberate energy from the reaction. Similarly, at the bottom, the anabolic enzymes induce a reaction which leads to the synthesis of protoplasm with the absorption of energy.

The enzymes of a cell are supposed to occupy positions on the surface of the cell or, according to Horning, upon the surface of a special part of the cell, the mitochondria. Since any cell must have a number of enzymes it follows that any living cell must be larger than an enzyme. The size of the enzyme molecule is thought to be of the order of 2 mμ, while the smallest size ascribed to any phage or virus, which may be supposed to be the smallest existing cells, is 20 mμ. It is possible that the smallness of the virus particle is one of the reasons for its parasitic existence in other cells; it is too small to house a full complement of enzymes itself and has to borrow the use of enzymes from its host.

We have already seen that two general types of enzyme processes are necessary in nutrition—a katabolic process and a synthetic or anabolic process.

The katabolic process depends upon the presence of enzymes which are capable of breaking down the substrate with the liberation of energy, and the anabolic upon the presence of enzymes which are capable of building up the substrate into protoplasm with the absorption of energy. The two processes are distinct; the katabolic process may be carried out in the test tube in the absence of a substrate suitable for anabolic enzymes, and thus, though energy is liberated, no nutrition of the cell can take place, since the anabolic enzymes cannot function.

Now there is evidence that the enzymes of a cell are to some extent inconstant constituents of the cell. Dr. Marjory Stephenson and her collaborators at Cambridge have shown that enzymes can be produced in bacteria in some circumstances, which are not present in other circumstances. A certain bacterium grown in the absence of formate does not possess enzymes capable of oxidizing formate, but the same organism, when grown in the presence of formate, *does* possess enzymes capable of oxidizing formate. Thus, the mere presence of a foreign substrate has caused new enzymes to appear. The same phenomenon was observed by Gorr and Wagner. A strain of Torula, when grown on asparagine, produced the enzyme asparaginase, but this was not produced when it was grown on ammonium sulphate.

These changes in the enzyme make-up of bacteria refer to bacteria of a lowly type, but there is reason to suppose that the same process may be demonstrated in pathogenic bacteria. Instances of change in the fermentative power of bacteria have been known for a long time. In 1907, Twort showed that paratyphoid bacilli could be induced to ferment saccharose, typhoid bacilli to ferment dulcite and lactose, and dysentery bacilli to ferment saccharose and lactose. Penfold made similar observations in 1910.

Time will not allow me to speculate upon the mechanism of this process, but I wish to establish that *enzymes can be produced in bacteria by the stimulation of an abnormal substrate.* If an enzyme is a chemical grouping which has a chemical affinity with its specific substrate, then you have a process analogous to the antibody and antigen familiar to students of immunity. The substrate, acting as an antigen, causes the production of the enzyme. To carry the comparison further, we may assume that a change in enzymes is rendered more easy if the chemical nature of the substrate is not altered too suddenly. A substrate with a slight chemical difference from the normal substrate will not be entirely without effect upon the normal enzyme-producing mechanism, but will cause a slight alteration in the enzyme, in the same way that antigens with slightly different structures exert a group effect upon the antibody-producing mechanism. A process of this sort may be taken to be the essence of what is known as the training of a microbe.

Now, let us consider the practical effect of altering the normal substrate of the enzymes of a microbe.

In the case of the katabolic process, as we have seen, the substrate may be altered to a greater or less extent or with greater or less rapidity, and the enzymes adapt themselves to the new conditions and liberate energy as before. The only effect of a failure to adapt will be a failure to liberate energy and therefore the nutrition of the cell fails.

When the anabolic process is considered, a failure to supply the particular substrate required by the enzymes of the cell will also lead to a failure in nutrition, and a successful adaptation of the enzymes to the altered substrate will lead to a successful nutrition, *but*—and here is the important point—the make-up of a cell which has been synthesized by changed enzymes from an abnormal substrate must be itself changed.

If a typhoid bacillus requires in its protoplasm the tryptophane nucleus, it must either find it in the substrate or possess enzymes which can synthesize it from a simpler nitrogen compound in the substrate; otherwise its nutrition fails. Now, the

typhoid bacillus does not possess these enzymes and therefore it cannot obtain nutrition, unless pre-formed tryptophane is present in the substrate. *B. pyocyaneus* also requires tryptophane in its protoplasm, but it has enzymes which can synthesize tryptophane from ammonia and, therefore, tryptophane need not be present in the substrate.

The typhoid bacillus is not, however, entirely helpless in the absence of tryptophane. It can, with greater or less difficulty, adapt itself to its absence by producing enzymes which can carry out the synthesis from ammonia to tryptophane. It can then grow (fig. 2).

The original typhoid bacillus was one which could synthesize its protoplasm only from complex *organic* mixtures; the new typhoid, on the other hand, can do so

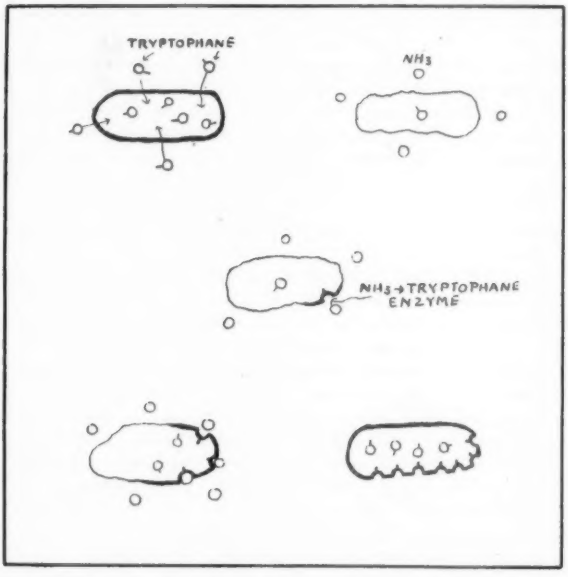

FIG. 2.—At the top (left) a normal typhoid bacillus is shown synthesizing its protoplasm from tryptophane in its environment, and (right) a typhoid bacillus in ammonia failing to flourish. In the centre this typhoid bacillus has started to react to the stimulus of ammonia and to produce a new enzyme. This process continues until the bacillus is fully provided with new enzymes capable of synthesizing the necessary tryptophane from ammonia.

from simple *inorganic* mixtures. The new typhoid, thus, in this respect is a different typhoid. It has different enzymes. The difference has been occasioned by an alteration in the nutritional environment. *The nutritional environment governs the precise enzymic make-up of a bacterium.*

Imagine now what would be the effect of broadcasting these microbes in their original and new form. You might be able to recover the new ones from clean ponds, but you could not recover the old ones from clean ponds because they could not grow there. The old ones could only be recovered from sources in which the tryptophane nucleus existed, that is, from animal and vegetable matter. Thus the old ones would tend to be associated in space with animals and the new ones need

not be associated with animals. Suppose one of the new ones did become associated with animals. What would be the effect? It then would not require its ammonia-tryptophane enzyme, and what would happen to it? It would in time fade away, even as an antibody fades away when there is no stimulus to preserve it. The new bacillus would then be like the old—associated only with animal matter.

Now let us proceed a stage further. Take this strain which has reverted again to an association with animals, deprive it of tryptophane and offer it ammonia. What happens? The stimulus of the ammonia will not be entirely foreign to it, the stimulus will be akin to the secondary stimulus of immunization and the ammonia-tryptophane enzyme will appear again more easily than it did before.

It appears possible to assume that when an organism adjusts itself to a simple substrate with which normally it cannot deal, it is because at one time in the history of the species it had been in the habit of using that substrate; that the process is a process of atavistic reversion.

It thus follows that an organism which possesses enzymes capable of synthesizing its protoplasm from simple inorganic sources is older, in an evolutionary sense, than an organism which can only synthesize itself from animal tissue.

Consideration of other aspects of the matter shows that this must be the case.

For this purpose I must picture for you the origin of life on this planet. Some speculative writers on this subject have, I think, failed to grasp the issue. H. G. Wells for instance states that life "began perhaps as a slime, as a sort of sub-life that slowly and imperceptibly took on the distinctive features of life. Upon no part of earth at present are there the sort of conditions, chemical and physical, under which life can conceivably have begun." The issue is first to imagine what was the state of the earth before life began, and then conclude what sort of life could exploit that state.

You must first determine the substrate because that governs the type of life which can exist on it.

I must, therefore, picture for you the earth before the beginnings of life: a dark earth with the light obscured by clouds, fogs of volcanic dust and torrential rains; hot steamy swamps containing in solution and suspension the washings of the surface. What sort of life could exist in these conditions—an atmosphere charged with volcanic gases, a hot surface of rock or solutions of inorganic salts? There is only one answer to this question—the autotrophic bacteria could live under these conditions, but no other form of life which we can picture.

The autotrophic bacteria are bacteria which are able to obtain their energy and building material from purely inorganic sources, and do not require organic matter or the co-operation of any other form of life. Among them are the nitrifying bacteria which play an important part in agriculture.

These organisms can oxidize such substrates as hydrogen, ammonia, carbon monoxide, hydrogen sulphide, sulphur, ferrous salts and water, and derive energy and material from them to grow and synthesize their own protoplasm, that is, synthesize all the highly complex amino-acids and other compounds of which their protoplasm is made up.

All these inorganic materials would be present in the muddy swamps which we have pictured and thus it follows that autotrophic bacteria could have been the first forms of life to develop, and could have been the first instances of the synthesis of organic material from inorganic.

They could, indeed, have existed before any vegetable matter requiring a photo-synthetic mechanism, because they could have existed before the sun's rays effectively penetrated the overhanging clouds—before there was light.

With the gradual establishment of a lowly form of life like the autotrophic bacteria, the constitution of the muddy pools in which they lived would become altered—they would no longer consist of purely inorganic substrates—the waste

products and the dead bodies of the bacteria themselves would introduce organic substrates.

The substrate determines the type of individual associated with it. Small changes in the substrate will induce small adjustments in the mechanisms operating on them, and these in turn will constitute small changes in the resulting individual. This is reaction to environment which, in conjunction with natural selection, leads to the major differences in genera and species.

I shall not be tempted to enlarge upon the necessary implications of this statement. Our fathers and grandfathers became frenzied at the idea that they were descended from apes, but we and our children, even if the case were stated with a brilliance equal to that of Huxley, would envisage an ascent from microbes with calm, if not with indifference. Outlooks have changed as species have changed.

To continue—these forces of change will operate upon the bacteria themselves as well as upon other forms of life subsequently evolved.

On the one hand, there will be the unchanged substrate of inorganic material and, on the other, a substrate of ever-increasing organic complexity. On the original inorganic substrate the autotrophic bacteria will continue to grow without change; on the ever-changing organic substrate the bacteria will be subject to continual change.

At the present time, therefore, we have representatives of the original primitive forms of life and specimens which have evolved from a precursor as a result of association with organic matter. The former are lower, the latter higher, in an evolutionary sense (fig. 3).

To digress for a moment again—it seems unjust to suggest, as one authority has suggested, that the autotrophic bacterium is a low form of life which leads a "hard and precarious existence." Let us look at the facts and compare its existence with that of its highly evolved relative, the gonococcus. The autotrophic bacterium has a range of enzymes of such importance that it can synthesize the most complex compounds from the simplest. It lives normally and indeed preferably under, to us, the most adverse conditions. The gonococcus, as shown by Barron and Miller, has a poor and ineffective range of enzymes and, of course, can only live under highly specialized conditions. In fact, the autotrophic bacterium enjoys a security of tenure upon this planet which no other form of life enjoys. If some extra-terrestrial catastrophe should tend to wipe out life on this planet, the autotrophic bacterium would probably be the only survivor. Thus we might be evolved again.

When, therefore, we say that the typhoid bacillus, which has acquired the power to synthesize its protoplasm from ammonia, is more primitive than one which has not, we mean that it has reverted to a condition nearer to that of an autotrophic bacterium, a condition which was normal to its progenitors, until an association with organic matter allowed it to drop the ammonia-tryptophane synthesis. When this power of synthesis has been lost, the microbe of course becomes increasingly associated in space with tryptophane-containing organic matter and, since animals constitute an important supply of this, they become increasingly associated with animals—or parasitic.

We can, therefore, argue, that the character of parasitism in a pathogenic microbe is associated with nutritional requirements of a certain order. The outstanding feature of this type of nutrition is that the microbe takes a number of complex substrates direct from its environment.

Now, since it is common knowledge that a microbe, on being deprived of these complex substrates in artificial cultivation, tends to become non-pathogenic, we may conclude that pathogenicity is associated with the taking of complex substrates from the host by the bacterium. But is this association accidental?

Let us take the case of the pneumococcus. This organism, as found, for instance, in a case of a mouse septicæmia, will not grow upon ordinary agar, but it can be

made to grow by mixing with the agar decreasing quantities of blood. The explanation of this phenomenon is that the microbe in the blood-stream finds conditions or materials which allow it to grow, but upon ordinary agar these conditions or substances are absent. It has, therefore, to adapt itself or undergo a variation to utilize such materials as are in the agar to allow it to grow, in place of the more convenient materials of the blood. We suppose the adaptation to consist of the development of new enzymes which can synthesize materials which existed pre-formed in the blood, or of the development of enzymes which can deal with materials in the different conditions of the agar in the same way as they were dealt with in the blood. Broadly, these two suppositions amount to the same thing.

Now associated with this adaptation to agar is a loss of power to grow in the blood of the mouse. The agar strain becomes non-virulent. The virulence of a

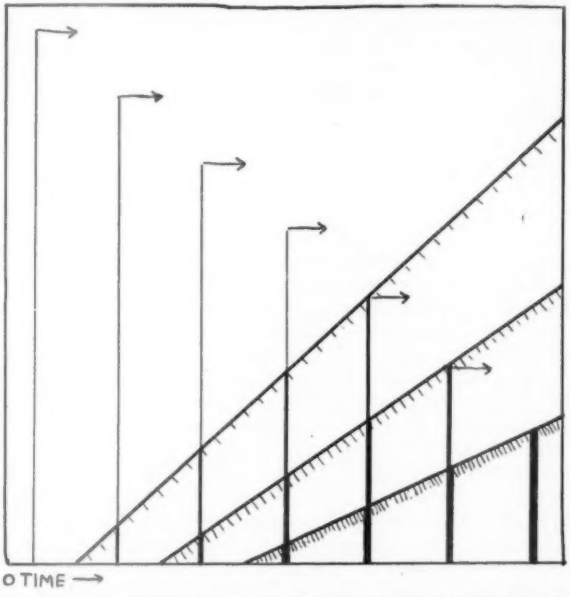

FIG. 3.—The sloping lines represent three samples of organic substrate increasing in organic complexity and concentration with time. The vertical lines represent characteristic bacteria of the times indicated. After zero time come the autotrophic bacteria requiring only inorganic materials. At later dates, bacteria are evolved which are more and more obligately associated with organic matter of a high degree of complexity. All these types of bacteria persist to date because the conditions of their evolution persist to date.

pneumococcus is retained in artificial culture by growing it in blood and so preventing the adaptation.

Further argument along these lines must become increasingly speculative. Is the loss of virulence in any way due to the fact that the microbe can make its own growth materials from simpler substrates, and therefore does not take them from the tissues of the host? Has the taking of growth materials from the host any bearing on virulence?

It seems quite conceivable that some part of the virulence process may be due to a deprivation of the host's cells of an essential substance by implanted bacteria; in fact, the disease produced may be in some part a deficiency disease.

One can picture a host's cell, when deprived of an essential substance, as being unable to carry out its proper functions, and among these the exploitation of anti-mechanisms in the widest sense. The sort of effect I have in mind is that of vitamin A in xerophthalmia. Here removal of vitamin A from the diet of the rat permits the invasion of the corneal epithelium by bacteria. Similarly, it is possible that removal by implanted bacteria of a substance necessary to themselves and to the tissues would so damage the tissues as to allow the bacteria to grow progressively in them.

Relying upon this argument, it might eventually be possible to influence the course of a virulent bacterial infection by supplying the essential substance required by the body cell and of which it has been deprived by the infecting bacterium.

However, as I have said, these arguments are purely speculative. They serve a useful purpose for the medical bacteriologist who is engaged in work of this sort. The nature of the work requires that he should withdraw somewhat from immediate medical interests, and, in so doing, he wishes to keep in mind that the fundamental object of the work is not the collection of abstract data, but the advance of medical knowledge. Such speculations as these remind him that his withdrawal is with the object of advancing further.

Section of Anæsthetics

President—H. P. CRAMPTON, M.D.

[*November* 2, 1934]

Factors, other than Anæsthetics, affecting Anæsthesia

PRESIDENT'S ADDRESS

By H. P. CRAMPTON, M.D.

THE modern anæsthetist has more anæsthetic drugs at his command than ever before, but there remain, and always will remain, certain factors influencing anæsthesia, irrespective of the drugs, the route, or the apparatus employed.

An obstructed airway is probably the most striking example. Even if one defeats the consequent rigid abdomen with spinal or infiltration aids, there remain the tumultuous breathing, venous oozing, and, in extreme cases, danger to life from anoxæmia.

Some of these factors are trivial, but the sum total of many trivialities may be large enough to make a real difference to anæsthesia, especially if the safety margin is small.

It will be convenient to discuss these factors in three groups: (1) The patient; (2) the anæsthetist; (3) the surgeon and the operation.

THE PATIENT

Mentality, physique, habits, and certain pathological conditions are among many factors influencing anæsthesia, no matter what drugs are employed.

A slender, placid girl of 20, is more easily anæsthetized than a fat and flustered woman of 50, the latter tending to be short-winded and easily cyanosed.

Mentality is, however, more important than physique. Large physique, *per se*, is no drawback. Some of my most satisfactory patients were men of the Guards Regiment, men of large physique, but magnificent discipline; while slender patients with poor moral control have been difficult patients—rigidity, jerky breathing, and other undesirable nervous phenomena lasting far into anæsthesia. The patient's mentality undoubtedly affects anæsthesia. We can render a patient unconscious in a matter of seconds, but "frightened" respiratory, cardiac, and spasmodic effects may persist afterwards, though rarely for more than half an hour. It is surprising how few people are in a state of real panic at the zero hour, but if real panic does persist it adds a definite risk to any mode of induction. Most patients are mildly apprehensive. Those who boldly admit their fear are, as a rule, quicker in reaching a smooth anæsthesia than those who suppress it. The latter, once their controlling centres are out of action, manifest the usual nervous signs in an exaggerated form. One of the worst types of mentality is that of the man who has long been in a position of absolute authority, never submitting his will to others. The same statement applies to the spoilt child and the pampered woman. Gwathmey states that the resistance of criminals is remarkable. To the best of my knowledge I have no personal experience of that class!

There is a certain type of patient in whom there appears to be no obvious contra-indication to anæsthesia, but, who, none the less, often "crumples up," just as surgical anæsthesia is reached. Induction seems to be progressing normally when, quite suddenly and for no apparent reason, the patient seems to collapse, despite the fact that the operation has not started and that overdose is unthinkable. One may notice this condition in the precociously developed and the infantile type of child, in patients who are in a real panic, and in those without the will to live (for example, the unsuccessful suicide).

Bad habits affect both mind and body and so influence anæsthesia.

Alcoholics require large doses and recover consciousness very quickly; they are seldom sick afterwards. I recognize two distinct types of alcoholic: (1) The cheery soul who drinks to excess in company. He comes up smiling for his anæsthesia, takes large quantities and may be noisy during induction, but, once he is "under," he sleeps like a babe, and is really quite a satisfactory patient. (2) The other type drinks to excess because he lacks moral courage to resist. He exhibits the usual nervous signs extending far into anæsthesia. The former group takes exercise; the latter group is probably fat and flabby, short-winded and sweats easily, not good material for any form of anæsthesia.

In passing, I may say that if I am having difficulty in defeating some patient I am inclined to excuse myself on the plea that "the man *must* be an alcoholic." If he recovers consciousness much sooner than usual I am probably justified in my excuse, otherwise I am probably wrong.

Tobacco fiends often have an irritable cough reflex, necessitating an unusual depth of anæsthesia. Moreover, they frequently suffer from vague cardiac sensations which convince them that they are not strong enough for an operation, and thus they become mentally resistant to anæsthesia.

I have had very little experience of drug addicts but I understand that they are very refractory.

Anæmia, fever, exhaustion, and shock are conditions promoting an easy anæsthesia, but the margin of safety is smaller. Fortunately for anæsthetists, the bad operation risk more often "slumps" after the cessation of—rather than during full—anæsthesia. In desperate cases I prefer to continue anæsthesia—if necessary in the lift—rather than stop before the end of the operation. This gives a better chance of getting the patient back to bed, where restorative measures are the more easily applied. Large abdominal tumours, ascites, and growths in the neck are examples of mechanical difficulties.

Fat has been described as the surgeon's worst enemy, and it can hardly be called a friend of the anæsthetist. Excessive fat is the result of some pathological process or lack of training. In either case the added difficulties to anæsthesia are too well known to need further comment. Pathological conditions liable to cause sudden death, such as status lymphaticus, myocardial degenerations, emboli, &c., are always possible factors prejudicing anæsthesia.

The Anæsthetist

Apart from premedication, basal narcotics or routine anæsthetic agents, the anæsthetist can bring to bear on anæsthesia at least three factors: (1) Stage management; (2) Judgment; (3) Personality. Without these attributes he remains a mere retailer of dope.

Stage management.—If anæsthesia is to leave nothing to chance it will be wise to stage-manage the preparation and induction stages. I propose to mention a few examples only.

Rest before anæsthesia is frequently not long enough. Rest makes for better muscular relaxation and mental submission but the patient must be given the means of mild mental occupation. For myself I should choose a light novel, a pack of patience cards and some easy cross-word puzzles.

It may be argued that patients needing sudden emergency operations usually take anæsthetics very well, but surely this is because they are mostly in pain and welcome the relief of oblivion.

I have given gas to doctors who had suffered from toothache or a boil overnight. With the morning courage returned and they felt that they must get through their day's work. By 6 or 7 p.m., tired out with loss of sleep and a day's work, they surrendered. The result is that they wake from their gas "green and flabby."

Do not knock a man off his usual tobacco or alcohol the night before anæsthesia. To do so will only make him miserable and irritable. If he smokes or drinks to excess, by all means begin a "cure" after anæsthesia, but efforts to wean him twenty-four, or even forty-eight, hours before operation are too belated to make any real difference for good.

At a meeting of the Section in 1923, Dr. W. J. McCardie read a paper on "General Anæsthesia in Dental Surgery."[1] Discussing preliminary sedative treatment, mostly for out-patients, the author said :—

". . . Given immediately before anæsthesia, brandy or whiskey are the only sure sedatives. If administered neat and freely, they will sometimes work wonders, even in alcoholics. For instance, a man, aged 30, who said he had had gas before and could not be put under, drank twelve brandies and sodas within one hour before the operation. Anæsthesia by the prolonged method was perfect and was continued between two and three minutes. He had very pleasant dreams."

Dr. McCardie has written in the margin of my reprint—"The moral is to give plenty of spirits and give them neat."

I am not advocating alcohol for all, but do believe that the patients' habits should be altered as little as possible on the eve of anæsthesia.

Stomach-tubes, enemas, purgatives, and starvation may sometimes be necessary, but are very liable to exhaust and distress, to the detriment of anæsthesia. Stomach tubes should not be passed till the threshold of unconsciousness. At this moment the cough and swallow reflexes can protect in case of vomiting and the patient is spared a very trying experience.

"While tranquillity of mind and body go far to assist in the production of anæsthesia, anxiety and uneasiness will greatly retard its accomplishment" said Buxton. That is why the "ten minutes before we start" is so important, the more so if we have missed the opportunity of making friends with our victim beforehand.

Certain nursing homes spend money on adorning their walls with the motto "Silence is golden." Taciturnity is entirely wrong, so far as the anæsthetist is concerned, no matter whether he is about to attack the respiratory tract, a vein, the rectum, or the theca. It does not matter much what you say, but say *something*, partly to create an idea that this is a very easy matter, but also because most people derive comfort from a friendly voice on such occasions.

Case I.—I know of one doctor who had two similar anæsthetics at short intervals. The first anæsthetist was silent and the doctor felt a sense of loneliness as he "went under." On the second occasion the anæsthetist talked and the patient derived much comfort.

Conversation must be conducted in low tones, as hearing may be greatly accentuated during the induction.

Hearing returns early and an assurance that all is well, spoken directly into the patient's ear, probably penetrates even though he is apparently still unconscious. If this is so it is likely to help the onset of a natural sleep. I once recognized a nurse by her voice half an hour before becoming conscious from an ether anæsthesia, and this, in spite of the fact that her visit to the theatre was unexpected, and that she only put her head through the door behind me.

For bodily comfort let patients have as many pillows as they like and assume any position in reason. It is very easy to place them in the required position when they are unconscious. I know from personal experience how annoying it is to have a pillow arranged under the shoulders in order to extend the neck.

It is preferable to let the patient hold the nurse's hand rather than the reverse. The holding of the patient's hand by another is liable to be interpreted in the "half-under" state, as a means of restraint, and may cause an attempt to struggle. Any violent struggle means a loss of energy, and many a promising induction has been marred by injudicious premature movements.

1 *Proceedings*, 1923, xvi (Sect. Anæsth., 11).

Judgment.—Judgment in the choice and dosage of anæsthetics will obviously affect anæsthesia, but drugs are outside the scope of this paper. Judgment, apart from drugs, will be referred to later, under the heading of "The Surgeon and the Operation."

Personality.—This is a vague but important factor which can affect the patient profoundly. I suppose we all consider we have the correct personality, or we should try to ape someone else, only to fail dismally: there is no one rule. If a dozen anæsthetists be selected you will find a dozen different personalities, with but one thing in common—a way of handling their patients. Just as some great leaders have made timid men follow and perform valiant deeds, so most experienced anæsthetists can handle timid patients and make them rise to the occasion. Personality is non-toxic and does not throw any strain on the heart, liver or kidneys, nor does it depress respiration.

The raw anæsthetic clerk probably claps on a face-piece with a curt, if kindly, "Breathe in and out." He hopes for the best but fears the worst and so infects his victim with his own misgivings. It is more than a question of confidence—in a comparatively short time he will gain confidence and be able to administer a sound straight anæsthesia, but it may take ages to develop his anæsthetic personality.

What is the secret? Probably the gift of being able to see things from the patient's point of view and the fact that most people place us on a pedestal—at any rate *before* their anæsthesia!—and so are very susceptible to any suggestion.

It is well not to approach patients with set phrases but rather to cultivate the art of firing off an appropriate remark at the right moment.

Even if we impress our personality, a nurse can do much to augment or completely wipe out our good efforts. What chance has an anæsthetist when a nurse asks her patient "Why did you engage Mr. Blank? I've never even heard of him." On the other hand a nurse who tells her patient that she "is very lucky to have Mr. Blank, as his patients are never sick," may not be speaking the exact truth, but her remarks will be of some help.

The Surgeon and the Operation

Surgeons are human, and, on occasions have been known to hurry the induction or even bluff the anæsthetist into allowing the operation to start before the patient is ready. Mostly, this will result in worse than delay, for an abdomen, once insulted, will rarely forgive for the rest of that anæsthesia. A senior anæsthetist may ward off this premature attack but it is often very difficult for a junior man to do so. Fortunately, most surgeons realize the importance of waiting for an "anæsthetic balance," and will talk to the theatre sister or address the visitors till this ideal is reached. To demand anæsthesia for a pelvic operation and suddenly to explore the gall-bladder without warning is a sure way to upset the patient.

Distended intestines *will* out if they can, and their replacement, plus packing to retain them, may be very detrimental to smooth anæsthesia.

Case II.—The patient was a man with a vastly distended abdomen, which was opened by a liberal mid-line incision. An enormously dilated stomach protruded. The assistant made valiant efforts to replace the stomach and the patient apparently died. The attention of the surgeon was drawn to this very undesirable state of affairs and he immediately incised the stomach. A great rush of gas escaped and the apparent corpse made a rapid return to life.

The personal factor.—This may seem a far-fetched idea but it certainly can influence anæsthesia.

Mr. Scalpel likes Mr. Doper, and anæsthesia is mostly smooth and satisfactory. Mr. Scissors is somewhat temperamental and is antagonistic to Mr. Doper; it will be surprising if with this combination anæsthesia reaches the same peaceful standard.

The operation.—The operation cannot fail to affect anæsthesia in varying degrees. We all know that pulling, tearing, and stretching affect anæsthesia more than cutting

with a sharp knife does, and that upper abdominal reflexes are more powerful than pelvic, that a total gastrectomy will disturb more than removal of a lipoma, and that operations involving much hæmorrhage will call for a marked reduction in dosage—an easy matter with inhalation methods but not so simple with intravenous or rectal therapy.

Case III.—I once anæsthetized a man for a gastro-enterostomy with gas and open ether. He took enormous quantities of ether and was kept under with difficulty. About four hours later I was recalled. He was said to be bleeding and was very pale, with a rapid, small pulse and all the usual signs of hæmorrhage. This time, induction with open ether *on a single layer of lint* was easy and a very small quantity was required for maintenance of anæsthesia.

I wonder how much of this change was due to hæmorrhage and how much to residual ether from the afternoon.

Menstruation.—It is convenient to speak of menstruation here. In most cases it is a question of whether the patient minds putting up with more trouble at this time. In major breast operations it may be a very real added risk to anæsthesia. It is easy enough to pick up cut vessels but quite another thing to control oozing, and the latter may be very severe at such times.

Case IV.—A radical breast operation was carried out with C.E. mixture. Oozing was excessive throughout and death occurred three hours after operation. It was only then that we learnt that a "period" had started the night before, and that no one had been informed.

Before leaving the question of the effect of the operation on anæsthesia, consider for a moment the case of a death on the table during a severe operation. If we argue that the anæsthetic had nothing to do with the death we are liable to be asked if the patient would have died at that particular moment if we had not given the anæsthetic. In many cases the answer may well be that the patient would not be dead if given the same anæsthetic for the same time without the severe operation.

Position.—The position of the patient may introduce undesirable complications, varying from difficulty in maintaining a patent airway, to a fatality, as in faulty position for spinal anæsthesia. On the other hand, position may facilitate anæsthesia by making operative measures easier, as instanced by gall-bladder, kidney, and chest operations for which the patient is suitably placed.

In empyema and such operations an intelligent and willing dresser is preferable to any mechanical arm rest, offering as he does, much greater relief to the unaffected side of the chest from the considerable weight of the upper arm.

The Trendelenburg position mostly makes for safety and ease of anæsthesia but may cause considerable respiratory embarrassment in obese subjects under full narcosis. A sudden return to the horizontal position may be fraught with danger.

The rolling of a patient from side to side after deep anæsthesia or severe operations, in order to apply a many-tailed bandage or to remove the ambulance canvas from the bed, may cause profound depression. It is easy to lift a patient like a horizontal poker, dry him, put on the binder and remove the canvas, but it takes three people to do so.

It has been suggested that it is easier to induce anæsthesia in a patient approximating to the sitting position, on account of the lessened cerebral blood-supply, more nearly resembling that of sleep, and it is claimed that the "propped-up" position at the close of anæsthesia is less likely to entail sickness or giddiness, on the analogy of the man, who, having imbibed too much wine, is all right till he puts his head on the pillow, and then finds that the room goes round.

The time factor.—The time factor in anæsthesia offers food for reflection. It is mostly held that about 9 a.m. is better for the patient than the small hours of the morning, when vitality is lowest. The low vitality of the surgeon and anæsthetist in these small hours may well react on anæsthesia also.

The optimum time for anæsthesia in cases of severe trauma requires a nicety of judgment if the best results are to be obtained. As a rule such patients are capable of considerable improvement under suitable treatment but will deteriorate again once the optimum is reached. Striking examples of these cases were to be found in casualty-clearing-station work during the Great War where special groups of officers had charge of resuscitation work and were responsible for sending patients to the theatre at the peak phase.

The length of time spent on any operation and its effect on anæsthesia is a very variable factor. Is it better for anæsthesia to crowd a given number of hurt stimuli into a short space or to spread them over a longer period? I think it is impossible to give a single ruling; so much depends on the severity of the hurt stimuli and on the patient's general condition. In the case of a healthy patient subjected to, say, a varicose vein operation, it matters little whether anæsthesia is of half an hours', or, as in one case, three and three-quarter hours' duration. Time may, however, be an important factor. With war-time multiple gun-shot wounds the consensus of opinion favoured two, three, or even four, surgeons working on the same patient, at the same time. These patients were often hungry, cold and exhausted, so that the sooner to bed the better.

Another very difficult time-factor problem is presented when we find our patient steadily becoming worse under anæsthesia.

Case V.—The patient was a man suffering from a gun-shot wound in the abdomen. The abdomen was opened and the obvious lesion was repaired. At this stage the patient was quite alive but showed signs of having had enough. In spite of a warning, the surgeon thought fit to make a minute and lengthy examination of the rest of the abdomen. No further lesion was found and the patient died while the last stitch was being inserted.

This raises the difficult question "Who is to decide the time limit—the surgeon or the anæsthetist?" To continue some operations may mean a death on the table; to abandon some operations may mean robbing the patient of his only chance.

Pause for recovery.—A pause for recovery during operations involving very severe hurt may be the means of making a successful termination possible. A boxer may be on the point of collapse from severe punishment when the gong is sounded and one minute's respite saves him. He may then put up quite a good performance in the next round. I submit that the same idea holds good for patients.

Case VI.—A man undergoing the operation of œsophagectomy gradually became sick unto death as the surgeon's hand, inserted in the neck, tried to meet the other hand inserted through the abdomen—via the thorax. A pause for recovery enabled the patient to withstand a fresh onslaught and this procedure was repeated more than once. The œsophagus was removed whole and the patient left the table in good condition.

Without these pauses for recovery the patient would certainly have died on the table. This is an extreme example but I suggest that the pause for recovery might be put into practice more frequently in any severe operation.

Punctuality.—Just one more time-factor consideration—punctuality. In the course of a busy day, this is sometimes impossible, but the delay may have a very adverse effect on anæsthesia.

A certain patient, keyed up for the ordeal, is expecting an operation at 2 o'clock. Some form of premedication, perhaps, has already been administered and then a nurse breaks the news that the surgeon has been delayed and cannot operate till 5 o'clock.

Food has been timed for a 2 o'clock operation and it is now too late to give more. The premedication effect will wear off and the unfortunate victim will have to screw up courage all over again. Such eventualities militate against that ease of mind and body which makes for a smooth anæsthesia.